



Now, Lou Miller happened to be the top person in malariology in the National Institutes of Health, a very competent person. General Russell said, "Do you think you can get him?" I said, "I can ask him. He also happens to be the president-elect for the American Society of Tropical Medicine. I guess that doesn't make any difference. You think he's all right?" "Yes, he'd be all right."

General Russell said, "Who else would you like to have?" I said, "I'd like to have Jim Oliver." "Who in the hell is Jim Oliver?" I said, "Well, he happens to be the top person in this country on ticks and on tick-borne diseases. It seems to me that the armed forces ought to be interested in ticks and tick-borne diseases. It just happens he's also the president-elect for the American Entomological Society. Would he be all right on the committee?" "Well, if you can get him, he'd be all right." I said, "I think I can get him."

And we went on down my list. I wanted Bruce Eldridge because he is a very competent medical entomologist who was in charge of this program for the army for some years. Now he's a civilian on the Davis campus of the University of California. And I wanted John Edman, chair of the entomology department of the University of Massachusetts, who was particularly interested in leishmaniasis and other diseases that are insect-borne. Anyway, Russell finally said, "All right. If that's who you want for your committee, if you can get them to say yes, you can have them." [laughter]

Now, that shows you one way a committee gets elected, okay? I've had other committees that I've inherited, and I wasn't given any choice in who was on them. Generally speaking, I think most organizations are perfectly willing to have chairpersons suggest who might be the best person or persons to be added when they have vacancies on their committees. Sometimes you'll take over a committee, and within the next year or two you'll have a chance to change the membership. People generally are allowed to be on one of these committees for only three years, and then they have to go off. So whether it's the National Institutes of Health or the armed forces or any other organization, if you're asked to head up one of their committees, you get a lot of voice in its composition. After all, you want people who can work with you. That doesn't mean you always will agree.

I've been on endless committees, advisory capacities, for the armed forces, and still am. As an advisor, you're on a completely different basis from the people who are in the armed forces, because you're a civilian, and there's a difference between being a civilian and being in the armed forces. You're an outside person coming in to review things. There's a lot of concern by



staffs of these organizations about the competence of outside people.

You get none of the "perks" for your services as you would if you were in the armed forces. I've never been able to get a loan to help buy my house or anything like that. But I don't resent it at all. As a civilian, I frequently didn't get paid at all or as much as the people in uniform. The salary I was receiving from the university during World War II was much lower than if I'd been a commissioned officer. When I started, my salary was well under \$300 a month. You can't tell me that a lieutenant colonel in the army is getting less than that.

Hughes: Did you have children at that point?

Reeves: Yes. The first son, William C. Reeves, was born in 1943 in San Francisco. He's forty-seven now. My second son, Robert Flay Reeves, is forty-four and also was born in San Francisco. The third son, Terrance Moulton Reeves, is forty-one and was born in Berkeley.

Hughes: Your wife was no longer teaching, was she?

Reeves: No, she was taking care of the kids, and particularly me.

#### Hammon's Consultant Work for the Army

Hughes: When Hammon moved to Pittsburgh in 1949, he still continued to serve on these committees?

Reeves: His particular research interests at that time were in developing vaccines for Japanese B encephalitis, because he was no longer involved in field studies of the type we were doing here. He newly discovered the hemorrhagic fever and shock syndrome aspects of dengue fever when he was out in the Philippines for the army investigating a polio epidemic in the armed forces. He was called into the hospital to look at these peculiar cases of a very severe disease that was killing Filipino children. That immediately developed a new area of concern for the Commission on Viral and Rickettsial Diseases, as it was a form of dengue, a new disease in the sense of its severity. Before that, the textbooks said nobody ever died of dengue; they just wished they had because it hurt so much.

Then he did research in Thailand for the virus commission on a number of projects on dengue and continued attempts to develop a Japanese B vaccine by attenuating the virus. That vaccine project

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University History Series

William C. Reeves

ARBOVIROLOGIST AND PROFESSOR, UC BERKELEY SCHOOL OF PUBLIC HEALTH

With an Introduction by  
James L. Hardy and Marilyn M. Milby

Interviews Conducted by  
Sally Smith Hughes  
in 1990 and 1991

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of Northern California, the West, and the Nation. Oral history is a modern research technique involving an interviewee and an informed interviewer in spontaneous conversation. The taped record is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The resulting manuscript is typed in final form, indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

\*\*\*\*\*

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William Carlisle Reeves, 1970





Cataloguing information

REEVES, William C. (1916)

Professor of public health

Arbovirologist and Professor, UC Berkeley School of Public Health, 1993, ix, 686 pp.

Childhood, Riverside, California; education, University of California, Berkeley, Ph.D. (medical entomology), 1943, M.P.H. (epidemiology), 1949; research assistant and associate, Hooper Foundation, UCSF, 1941-1949; UC Berkeley School of Public Health: lecturer, associate professor, professor, 1946-1987, dean, 1967-1971, and head, Program in Epidemiology, Department of Biomedical and Environmental Health Sciences, 1975-1985; field and laboratory research on western equine and St. Louis encephalitis; California mosquito control programs; relations with Karl F. Meyer and William McD. Hammon; collaboration with Center for Disease Control; discussion of global warming and emerging viruses; development of arbovirology; consultant positions with national and international scientific and health organizations; history of and service at UC Berkeley School of Public Health.

Introduction by James L. Hardy and Marilyn M. Milby, School of Public Health, UC Berkeley.

Interviewed 1990 and 1991 by Sally Smith Hughes for the University History Oral History Series. The Regional Oral History Office, The Bancroft Library, University of California, Berkeley.





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## PREFACE

When President Robert Gordon Sproul proposed that the Regents of the University of California establish a Regional Oral History Office, he was eager to have the office document both the University's history and its impact on the state. The Regents established the office in 1954, "to tape record the memoirs of persons who have contributed significantly to the history of California and the West," thus embracing President Sproul's vision and expanding its scope.

Administratively, the new program at Berkeley was placed within the library, but the budget line was direct to the Office of the President. An Academic Senate committee served as executive. In the more than three decades that followed, the program has grown in scope and personnel, and has taken its place as a division of The Bancroft Library, the University's manuscript and rare books Library. The essential purpose of the office, however, remains as it was in the beginning: to document the movers and shakers of California and the West, and to give special attention to those who have strong and often continuing links to the University of California.

The Regional Oral History Office at Berkeley is the oldest such entity within the University system, and the University History series is the Regional Oral History Office's longest established series of memoirs. That series documents the institutional history of the University. It captures the flavor of incidents, events, personalities, and details that formal records cannot reach. It traces the contributions of graduates and faculty members, officers and staff in the statewide arena, and reveals the ways the University and the community have learned to deal with each other over time.

The University History series provides background in two areas. First is the external setting, the ways the University stimulates, serves, and responds to the community through research, publication, and the education of generalists and specialists. The other is the internal history that binds together University participants from a variety of eras and specialties, and reminds them of interests in common. For faculty, staff, and alumni, the University History memoirs serve as reminders of the work of predecessors, and foster a sense of responsibility toward those who will join the University in years to come. For those who are interviewed, the memoirs present a chance to express perceptions about the University and its role, and offer one's own legacy of memories to the University itself.

The University History series over the years has enjoyed financial support from a variety of sources. These include alumni groups and individuals, members of particular industries and those involved in specific subject fields, campus departments, administrative units and



special groups, as well as grants and private gifts. Some examples follow.

Professor Walton Bean, with the aid of Verne A. Stadtman, Centennial Editor, conducted a number of significant oral history memoirs in cooperation with the University's Centennial History Project (1968). More recently, the Women's Faculty Club supported a series on the club and its members in order to preserve insights into the role of women in the faculty, in research areas, and in administrative fields. Guided by Richard Erickson, the Alumni Association has supported a variety of interviews, including those with Ida Sproul, wife of the President; athletic coaches Clint Evans and Brutus Hamilton; and alumnus Jean Carter Witter.

The California Wine Industry Series reached to the University campus by featuring Professors Maynard A. Amerine and William V. Cruess, among others. Regent Elinor Heller was interviewed in the series on California Women Political Leaders, with support from the National Endowment for the Humanities; her oral history included an extensive discussion of her years with the University through interviews funded by her family's gift to the University.

On campus, the Friends of the East Asiatic Library and the UC Berkeley Foundation supported the memoir of Elizabeth Huff, the library's founder; the Water Resources Center provided for the interviews of Professors Percy H. McGaughey, Sidney T. Harding, and Wilfred Langelier. Their own academic units and friends joined to contribute for such memoirists as Dean Ewald T. Grether, Business Administration; Professor Garff Wilson, Public Ceremonies; Regents' Secretary Marjorie Woolman; and Dean Morrough P. O'Brien, Engineering.

As the class gift on their 50th Anniversary, the Class of 1931 endowed an oral history series titled "The University of California, Source of Community Leaders." These interviews will reflect President Sproul's vision by encompassing leadership both state- and nationwide, as well as in special fields, and will include memoirists from the University's alumni, faculty members and administrators. The first oral history focused on President Sproul himself. Interviews with 34 key individuals dealt with his career from student years in the early 1900s through his term as the University's 11th President, from 1930-1958.

More recently, University President David Pierpont Gardner has shown his interest in and support for oral histories, as a result of his own views and in harmony with President Sproul's original intent. The University History memoirs continue to document the life of the University and to link its community more closely -- Regents, alumni, faculty, staff members, and students. Through these oral history interviews, the University keeps its own history alive, along with the flavor of irreplaceable personal memories, experiences, and perceptions.



A full list of completed memoirs and those in process in the series is included in this volume.

The Regional Oral History Office is under the the direction of Willa K. Baum and under the administrative supervision of The Bancroft Library.

9 November 1987  
Regional Oral History Office  
University of California  
Berkeley, California

Harriet Nathan, Series Director  
University History Series

Willa K. Baum, Division Head  
Regional Oral History Office



## INTRODUCTION--by James L. Hardy and Marilyn M. Milby

William Carlisle Reeves is a towering individual, both physically--he stands well over six feet tall--and in the impact he has had on the students, professional colleagues and institutions he has been associated with throughout his long and illustrious career. In an eloquent introduction to the Symposium on The Epidemiology of Mosquito-Borne Virus Encephalitides in the United States, 1943-1987 which honored Bill Reeves' retirement from the University of California, his friend and fellow fisherman Dr. Karl M. Johnson described him as a "quintessential Californian" and a "true citizen-scientist". Both terms reflect his devotion to his native state, his nation, and his profession.

Bill Reeves' research career has been documented in great detail in this oral history as well as in his more than two hundred scientific publications and countless unpublished reports. His research achievements have been honored by the major professional organizations in the fields of tropical medicine, medical entomology and infectious disease epidemiology. The principal legacies of this research are the balanced field-laboratory approach to elucidate the transmission cycles of arboviruses and the California Encephalitis Surveillance Program. Both of these continue to serve as models for other arbovirus research and control programs in the United States and many other countries of the world.

His years of devoted service to the University of California, his profession, and various state, national and international organizations are widely recognized, as evidenced by the numerous prestigious awards he has received in the last decade. Anybody who has been associated with Bill Reeves knows that he is an active and productive member of any committee or study group on which he serves. He has the unique ability to quickly grasp all aspects of a problem, whether they be technical, theoretical or administrative, and to reach an insightful conclusion by concentrating on the critical issues or facts. Furthermore, he writes timely, lucid and detailed reports, with thoughtful and far-reaching recommendations, for any administrative assignment or consultantship he undertakes. Consequently, he has been asked to serve, frequently as chairman, on many prominent study groups that have had major impacts on research and administrative programs in California and the United States.

But Bill Reeves is first and foremost a teacher. His classroom lectures are memorable for his use of the Socratic method to involve students in unraveling the mysteries of outbreaks of infectious diseases. He diligently prepares each session, even those he has repeated time and time again, taking care to organize his visual aids and update them to reflect the most current knowledge of the disease he is describing. He

frequently brings in guest speakers to present current research on other infectious or chronic diseases of public health importance. These have included renowned scientists from around the world who were willing to come to Berkeley because of their admiration for him and his reputation as a leader in his field. In seminars he coaxes his students into greater awareness of the broader implications of the topics under discussion. He also is exemplary in the respect he shows for students, extending even to his habit of always wearing a coat and tie to teach or participate in a doctoral examination.

Whether grading student papers, editing reports, refereeing scientific manuscripts or reviewing drafts of doctoral dissertations, his careful attention to content, organization and detail is legendary. Many a dissertation draft has been returned to its creator looking like a "bloody mess", eliciting so much distress that Bill finally gave up his notorious red pencil. The two of us, after many years of trying to emulate the Reeves style, have yet to have him return an unmarked draft. His editorial suggestions have invariably improved the clarity, readability and grammatical and scientific acceptability of every document that has passed beneath his critical eye and hovering pencil.

Students and colleagues conducting research under his direction have a unique opportunity to learn, especially those whose work takes them into the field with him on collecting trips. His methods of searching for specimens have evolved over the years into a source of personal pride, and he can always be counted upon to find mosquitoes when everyone else in the group has decided to give up and return to the field station or laboratory. He brags about his ability to "think like a mosquito". Bill seems happiest and most relaxed when working out of doors, perhaps a reflection of his early life on the farm in Riverside. In these informal settings, he displays his wry sense of humor while telling stories about his most recent fishing trip or about places, projects, and people he has worked on or with over the years. Several years after his retirement, he replaced his old station wagon with a large 4-wheel-drive van to facilitate collecting trips to find mosquitoes that breed in snow pools in the Sierra Nevada. The van, like everything else he buys including wine, was American-made, because that's where his loyalties lie. It has served him well, gotten him and his colleagues out of snow and mud more than once, and brought home numerous "killer collections" of mosquitoes from remote areas. The license plate on the van says CULEX T, to honor the mosquito that has been the subject of his research for more than fifty years.

No one is more concerned about the professional development of his students, colleagues and research staff than Bill Reeves. Even though some of us found him aloof at first, we quickly learned that this was merely a facade to allow him to evaluate our potential before he made a commitment to us. Once this commitment was made, he became the best teacher and friend



a person could ever hope to have. He encourages each member of his research group to participate in the design of research projects and the development of new techniques as well as to discuss new ideas with him, and he is quick to support or adopt any and all that show promise. He frequently gives the nucleus of an idea for a research problem to a student or staff member and lets them develop it as their own research. Many of his ideas have been published by his students and research staff without his being a co-author on the paper because he did not want his name to overshadow the other authors and hinder their professional development. He considers this to be one of the most important roles of a teacher in a university.

In our combined fifty-five years of association with Bill Reeves, neither of us has ever seen him ask any staff member to do some menial task that he would not willingly do himself, be it Xeroxing a letter, cleaning out a chicken coop, or collecting adult mosquitoes from a privy. The only exception may be taking blood samples from small vertebrates, because he has recognized that his bleeding skills have long since been surpassed by those of at least two members of the field staff. After years of denying his ability to type, he recently purchased his first home computer. Of course, it's still in the box, but spring is a busy season for him, with many orchids to be repotted and snow pool mosquitoes to be collected.

Bill's loyalty to and respect for his research staff and other professional colleagues are perhaps the most endearing facets of this remarkable personality. Behind the sometimes gruff exterior is a sensitive and caring friend. At the time of his retirement dinner, he insisted on reviewing the list of people who had made reservations because he wanted to avoid embarrassing himself or any of the guests by failing to remember someone's name. He found one name on the list of about 170 that he did not recognize, that of State Senator Boatwright's administrative aide who was going to present him with a commendation from the California Legislature. This was scheduled to be a surprise for him, so we covered by saying the man must be some student's date, but Bill was uneasy because he thought he knew all his students' current boyfriends or girlfriends.

It has been our privilege to be a part of the Reeves team.

James L. Hardy and Marilyn M. Milby  
Department of Public Health  
University of California

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Berkeley, California



## INTERVIEW HISTORY--William C. Reeves

Anyone who knows arbovirology--the study of viruses borne by arthropods (in this case, insects)--knows William Carlisle Reeves in person or by reputation. Anyone who studies western equine encephalitis (and/or St. Louis encephalitis) is indebted to him for his pioneering work on these mosquito-borne diseases, formerly epidemic in the western United States. Anyone who has studied epidemiology at the University of California School of Public Health in the past forty-plus years recalls this dynamic, demanding, and imposing teacher who received the University's Distinguished Teaching Award in 1981. Anyone associated with the school in the late 1960s and early 1970s remembers Dr. Reeves' firm control during the politically troubled years of his deanship (1967-1971).

Drs. Evelynne and David Lennette, who have initiated and helped to underwrite ROHO's oral histories of E.H. Lennette and Harald N. Johnson, virologists with the Rockefeller Foundation and the California Department of Public Health, suggested and helped to underwrite the oral history of Dr. Reeves. Twelve interview sessions with Dr. Reeves were launched in November 1990 and concluded in July 1991. We met once or twice a month in Dr. Reeves's campus office where he invariably presented me with assorted documents relevant to the oral history. Frank, friendly, and expansive, he talked at length about all aspects of his career, never reluctant to insert a trenchant comment or witty remark, sometimes at the interviewer's expense.

Highlights of the oral history are Dr. Reeves's single-minded focus on unravelling every aspect of the life history of the viruses causing western equine and St. Louis encephalitis, which were epidemic in California and Washington when he began his graduate work in the late 1930s. An entomologist, his research brought him into contact with people from many different disciplines and taught him the value of a multidisciplinary team approach. The most eminent of Dr. Reeves' early associates was Karl F. Meyer, director of the Hooper Foundation at the University of California, San Francisco, where Dr. Reeves was a research assistant from 1941-1949. His tales of "K.F." supplement the Regional Oral History Office's volume on Dr. Meyer.

In addition to a detailed account of encephalitis research and the growth of the field of arbovirology, Dr. Reeves tells of his committee work, his consultant positions with governmental and scientific agencies, and his reluctant tenure as dean of the School of Public Health. Because of his long association with the school, he is able to provide an extensive history which includes the school's predecessor, the Department of Hygiene.

Working hard and fast, Dr. Reeves meticulously edited and made additions to the interview transcripts. He thrice reviewed the entire oral history and persuaded Marilyn Milby, a biostatistician and long-time associate, to proof it. We are very grateful to both of them.

We also thank the Lennettes for providing continuing financial support and James Hardy, Ph.D., of the School of Public Health, and the late John Combs, president of the California Mosquito and Vector Control Association, and his successor Donald E. Eliason, Dr.P.H., for soliciting donations from their respective colleagues.

James Hardy, Marilyn Milby, and Joyce Lashof, Ph.D., dean emeritus of the School of Public Health, graciously agreed to background interviews, the tapes of which are on deposit in The Bancroft Library. Dr. Hardy, with the indispensable assistance of Marilyn Milby, wrote the introduction, which is based on their many decades of research collaboration and friendship with Dr. Reeves.

Of interest of scholars is the fact that Dr. Reeves' extensive correspondence will eventually be available for research at The Bancroft Library.

As the oral history fully documents, Dr. Reeves's contributions are numerous and on many levels. An entomologist turned virologist turned epidemiologist, he is known for his focus for over fifty years on the viral encephalitides. He has also played a major role in national and international agencies and contributed to his beloved School of Public Health as teacher and dean. Throughout his career, Dr. Reeves has worked closely with mosquito control districts up and down the state of California. These accomplishments, combined with his over six-foot height and penetrating wit, could make him an intimidating figure. Instead, one encounters a very human, down-to-earth, fun-loving individual. We hope these attributes, as well as his many contributions, are evident in this history of the affable, unaffected, and thoroughly likeable Bill Reeves.

Sally Smith Hughes, Ph.D.  
Interviewer/Editor

June 1992  
Regional Oral History Office  
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University of California, Berkeley



## I FAMILY BACKGROUND, YOUTH, AND EDUCATION

[Interview 1: November 14, 1990]##<sup>1</sup>

### Childhood on a Ranch in Riverside, California

Hughes: Please tell me about your family background<sup>2</sup> as far back as you can remember. I have your family tree, if you want to refer to that.

Reeves: Actually, our family doesn't have a great deal of history that I'm familiar with. The reason for that is that I was an only son; my mother had a very difficult time when I was born, so I had no siblings. We lived in sort of an isolated place on a small ranch near a rural community called Riverside, California. A ranch was a hard way to make a living in the time that I can remember, the twenties and the thirties, which weren't exactly the most wealthy times in the United States. So we just lived off the ranch.

Hughes: What were you raising?

Reeves: My father was a beekeeper. He had about two hundred stands of bees. On the ranch we had oranges, walnuts, cherries, peaches, a large garden, and a lot of chickens, guinea fowl, and ducks that ran around loose till we ate them. So we really lived off of the property. I can remember my folks saying during the twenties, "We don't have enough money for this, we don't have enough money for that." To a child it meant nothing, but I can always remember them saying that. We were poor as far as money was concerned, but we were rich in other ways. We had all we wanted to eat, we ate

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<sup>1</sup>This symbol (##) indicates that a tape or segment of a tape has begun or ended. For a guide to the tapes, please see end of interview.

<sup>2</sup>See Appendix A for the Reeves family tree.

very well, and we were very busy. My father was an avid hunter and fisherman, so we spent a lot of time together doing that and eating the catch. So it was a very interesting rural life.

If you look, now, where our ranch was then, at the corner of Central and Riverside avenues in Riverside, it is nothing but supermarkets, gasoline stations, and asphalt. I walked down Central Avenue, a dirt street going to Palm Grammar School, and I walked barefoot. There was no requirement to wear shoes at that time in grammar school if the weather permitted you not to. We saved a lot of leather that way. All you had to do was be careful if you walked in six inches of dust that you didn't stub your toe on a rock. Anyway, it was a very rigid country school. The teacher in the first grade had a great big ruler, and she didn't hesitate to use it at all. I guess the teacher had to do that to handle farm kids.

Basically we weren't living in a community sort of a situation where I had a lot of contact with other children or city life. The closest ranch was a quarter mile down the road. The only other boy of my age in the area lived a good half mile away.

Hughes: Did you have a lot of chores when you came home from school?

Reeves: I was made to do everything I could do that would help, and I was expected to. I never argued about it, because that's the way you grew up. So I was milking cows and hoeing fields when I was old enough to stand up or sit down and do it. When I was, say, high school age, I spent the summers extracting honey and being stung by bees. My dad kept trying to get me interested in beekeeping. He said I could take care of a few hives of bees, and what they made would be mine; I'd get the money from them. My response was, "Forget it. I'll extract honey, I'll help you move them, but I don't care about bees."

Hughes: Was he hoping that you would follow along in the business?

Reeves: Actually, no. Well, my folks were not educated. My mother had only gone through third grade. It's amazing, because her father was a school teacher, but he didn't believe that girls in Tennessee should go to school. And my father had gone through sixth grade and beyond that was self educated by reading and experience.

Hughes: Where was he from?

Reeves: He was from Bethany, Ohio. My mother was from Gallatin, Tennessee. They were very concerned that I go into what I would guess you'd call between a blue- and a white-collar sort of a

situation. Their idea of utopia would be for their son to be an accountant or something, so you were assured of a salary. They even tried to force me to go to business school a couple of summers. It didn't work out. I got thrown out of school.

Hughes: Because you hated it?

Reeves: I didn't like it at all. I didn't mind learning typing. That was all right, because I knew I'd probably use that later. I got thrown out of the school when the teacher left the room in a typing class and I put the record "The Carioca" on the victrola-- "Flying Down to Rio" was the current movie, and I liked the music and had bought the record. When the teacher left the room she had a march on to provide the timing for us to type by, you know, Da ta ta ta on the typewriter, and I took it off and put "The Carioca" on. You know, Dadadada. [laughter] You type pretty fast that way. You got up to sixty words a minute very rapidly. But she broke the record and threw me out of the school because I was disruptive. I had to walk two and a half miles home because it was that far from town to the ranch. I won't go into the details of my reception and explanations. It was in summer, and it was 100 degrees in the shade. But I was so glad to get out of there that I didn't care.

Hughes: Did your classmates have similar distances to walk?

Reeves: We didn't have school buses in those days. The grammar school I went to was only about a half mile or a mile from home, but then high school and junior high school were two and a half miles away. It was a good walk.

Hughes: What kind of a student were you?

Reeves: I didn't really care particularly one way or the other about academics. I guess I was all right, but I didn't work hard. I didn't work hard at studying until I came up to Berkeley. I had decent grades or I couldn't have gotten into Berkeley after two years at junior college. I had no difficulty, but I wasn't a student. I was doing athletics and was too interested in chasing bugs in my spare time.

### Grandparents and Parents

Hughes: What do you remember of your grandparents?



Reeves: One time when I was very small--I must have been only three or four years old--I was taken to Ohio. I had a grandmother [Virginia Muchmore Reeves] on my father's side who was alive then. I don't remember her at all. One time the grandmother [Ollie Pryor Brant] from Tennessee came to Riverside and visited us, and I only remember one thing, and that was that she scared me because she had false teeth and I didn't know there were such things. When I did something she didn't like, she would shove the false teeth out with her tongue. I was a little kid about three or four years old, and I will never forget seeing that. I thought teeth were stationary, and to have somebody shove their teeth out and click them absolutely did me in. It scared me.

We made one visit back to their farm in Tennessee. I have a very vague recollection; I was no more than four years old. My uncle [Carlisle Brant] was still alive at that time and ran the farm, and my mother's mother was still alive. All I remember is being chased by a cow that didn't like the red jacket I had on. I remember I thought I was a goner, but my uncle got the cow by the tail and dug his heels in, and I escaped the cow. That is really about all I remember about the visit. Oh, I saw my first snow in Ohio at that time too and built a snowman, I remember that.

Hughes: How did your parents [Abie Bessie Harriet Brant and William Claude Reeves] meet?

Reeves: I don't know. I never asked them, and I don't know why I never asked them. He lived in Ohio and she was in Tennessee, and that's quite a distance apart. My father came out to California in 1913, and they weren't married at that time. He came to Riverside. He knew a family there by the name of Boyd and went to visit them to see if he liked this part of the country before he settled down. He spent a little time in Riverside, and then he made a very interesting trip. He took a narrow-gauge railroad through Randsberg up to Bishop, California, and met some fellows from Los Angeles. They were going on a backpacking trip into the High Sierras. There were no roads, no trails, and they spent a month on the trip, coming out at Mammoth Lakes. In later years, when he and I were fishing this area, he would take me to places where he'd done various things, which was interesting. Currently, Dr. Bruce Eldridge and I are doing research on snow mosquitoes in this same territory.

Hughes: So your dad was a real outdoorsman.

Reeves: Yes, he was a very good hunter and fisherman.

Hughes: To get back to your father and mother--

Reeves: My father bought this small ranch that I grew up on. I assume he wrote to my mother and had her come out and join him, and they got married.

Hughes: Both sides were farming families?

Reeves: Both sides were farming families.

Hughes: Do you know why he chose to come to California?

Reeves: I really don't know why he did. His father, William Claude Reeves, had gone to California in 1857 via the Isthmus of Panama. He farmed in southern California and probably made friends there. Then in 1862 he moved to Salt Lake City, where he ran a mercantile store for five years and then ran cattle ranches in Nevada and Utah. He returned to Ohio in 1876. One of my boys has his diary now, in which he listed the price of merchandise. I assume he talked to my father about his experiences and perhaps gave him the names of people in Riverside, California.

Hughes: He wasn't a Mormon?

Reeves: No, he wasn't a Mormon.

I grew up knowing that I had no living relatives that were anything more than distant cousins, whom I never met. So on my side of the family there was never any family relationship of that type. My wife, in contrast, had eight aunts and umpteen relatives, but I never had that pleasure. It didn't bother me any. Everybody told me I was spoiled because of being an only child, but I think I got more individual attention that I didn't want than anybody else [laughing] in the valley.

Hughes: You got all the work, too. There was no younger brother to shove it off on.

Reeves: No younger or older.

Hughes: Tell me something about your parents as personalities. What was your father like?<sup>1</sup>

Reeves: He was a very quiet person, but he believed in right and wrong. Until somebody crossed the path that made something wrong, he would say nothing about anything. He was respected as the sort of a person that was very cooperative with everybody else, and

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<sup>1</sup>The remainder of this section was moved here from its original position later in the transcript of interview 1.

everyone liked him, with few exceptions. But he was a very tough person when things got rough. He was much smaller than I am. He was about five foot eight. My mother was taller than he was. She was the opposite. She was a very outgoing, domineering, involved individual. So they made an interesting couple.

Hughes: Were you close to them?

Reeves: I don't know what you mean by "close." I was never an overly affectionate person, and they weren't either. But we got along together very well, and there were no problems most of the time. They were very strict, but that was what society was like then. I wasn't allowed to drive a car--except when I was working on the ranch--until I was a senior in high school. I wasn't allowed to have a driver's license, and it wasn't necessary. You could always walk to town or ride a bicycle. I drove a Model T all over the ranch when I was working, but I wasn't allowed to get a driver's license to drive on the street.

Hughes: Anything else about your relationship with your parents?

Reeves: My mother was always taking the bit in her teeth and making decisions, and she decided that I ought to learn how to swim. It was a little hard to learn how to swim out where we lived; there wasn't any water around, just irrigation water. I must have been around ten years old. One Saturday she put me in the car, and we went up to the YMCA and went up to the secretary, Mr. Caldwell. She said, "This is my son, Billy." She knew him already, because she knew everybody in town. "I want him to learn how to swim. He's going to be a member of your YMCA, and I'm going to deliver him every Saturday morning, and I'll pick him up." My mother didn't know how to swim. She never could learn, and she tried many, many times--she took lessons--and she was damn sure I was going to learn how to swim. I didn't learn in the swimming classes, because that was too organized, but you learned when you fell in the deep end and you had to get out some way. So the YMCA became really a very large part of my life at that time.

Hughes: Just for swimming, or for other things as well?

Reeves: Basketball, but I hated gymnastics. I wasn't coordinated enough to do backflips and all those things. The short, stocky kids could do all that stuff. I never learned to stand on my head or to do handstands. Parallel bars terrified me. But the other stuff, the swimming and the basketball, were big stuff. So I became very active in the YMCA. This was really where I learned to tolerate, get along with other people.

Hughes: Was there anybody in your life that served as a mentor?



Reeves: Not until junior high school, when I got interested in science. Meanwhile, in what we called our neighborhood, there were the other people that lived on farms around us, and they represented our social life.

It was a very rural life. When I graduated from high school in '34, I think there were only 28,000 people in Riverside, and now it is 300,000-400,000 population.

Hughes: When and why did it begin to boom?

Reeves: It boomed in part during the war because March Field was a big air base that's near there, and Riverside was the closest town to the base. Then Kaiser Steel came into the region, and a lot of small industry developed during the war. There also was a big cement factory located near Riverside. I think the town also just got caught up in the building boom because it's so close to Los Angeles. There was a lot of spillover of people from that big metropolitan area. Agriculture was the only real industry in Riverside at the time I grew up. There's a fair amount of small industries there now.

Hughes: Had the farming families that lived around you recently moved in?

Reeves: They were mostly families that had come there just before or a generation before my father.

Hughes: So your family was relatively recent?

Reeves: Well, they were recent when they moved in. By the time I was growing up in Riverside, they were an old family as far as Riverside was concerned.

It was a very small community. There was only one high school in the area; it was a polytechnic high school. They didn't have any separate junior high schools. I went to Central Junior High School the first year it was open. Before then they had no junior high schools that were organized as such. It was just an extended grammar school, and then you went on to high school.

My mother decided that I should go to kindergarten, probably to get me out of her way. She had to take me up into the town area where more people lived, because there was no kindergarten near where I later went to grammar school. Kindergarten was a terrifying thing to me, because I'd grown up on a farm. I hadn't had much contact with other kids, and I found having all these city kids around me was sort of a bother. I didn't enjoy it

particularly. Well, I made some friends that I went through high school with later.

Hughes: So you kept a cluster of friends that followed you all the way through school?

Reeves: Yes, because it was a small community. You went downtown, you walked down the street, you knew everyone. You get a town like that, between 20,000 and 30,000 people, and there aren't very many people you don't know one way or the other. But you separated into city people and country people. It seemed to me to be a very strict division, socially.

Hughes: The city people looking down their noses at the country people?

Reeves: To some degree.

Hughes: And vice versa?

Reeves: No. We didn't care. [laughter] My wife [Mary Jane Moulton] was a city girl, and her father [Flay Edwin Moulton] was the head of the Southern California Automobile Club for Riverside County. She was a member of the social elite in the city, and they belonged to the golf club and all that sort of thing, but those worlds didn't come together until high school. I felt there was still a lot of separation even in high school.

Hughes: Were you unusual in bridging that gap?

Reeves: I don't think so. No, I was only interested in sports. I wasn't interested in what those people thought.

### Religion and Politics

Hughes: What about religion?

Reeves: The neighbors who had the closest ranch to us went to the Baptist church religiously and took their two sons and daughter, so I got dragged into that. My folks didn't go to church at all, but they thought it would be good for me to go. And it was a real rolling Baptist church. [laughter] The minister's name was Catherwood, and we nicknamed him "Scattergood." You went to Sunday school but were dragged into the church and baptized and anything else they could talk you into. [laughter]

Hughes: The way you're saying this, I don't think it took.

Reeves: I didn't take it too seriously. The difficulty was that I knew a lot of the people in the church, and they lived on nearby farms. They were cursing people who didn't live by what we were taught in the church that you shouldn't do. As a matter of fact, my folks belonged to a lodge, and practically every Saturday night they'd have a dance. Catherwood would get up on Sunday and say that was sinful. It was a little hard for me to bridge the fact that on Saturday night I went with my folks and had a great time at these social functions, and then Sunday I was told it was bad. I couldn't quite agree to that, so I didn't take Catherwood too seriously.

Hughes: What about politics?

Reeves: There were no politics in Riverside. It was Republican, period. I didn't know anyone who would admit they were a Democrat until, I would say, I was probably through high school. Everybody in Riverside was a Republican. Well, we exported some of them to Orange County [laughter].

Hughes: Yes, I would say so.

Reeves: They were Republicans, and they might be Ku Klux Klans, because a number of people were, but they didn't talk about that, either. But Ku Klux Klans were quite active in that area at that time.

Hughes: Were there minorities?

Reeves: Well, that was interesting. Yes, there were several minority groups in Riverside, and we went to school with them all the time.

There was Casablanca, which was a little suburb of Riverside and entirely Mexican. We didn't call them anything other than that in those days, and it was not a derogatory term at all. We got along with these people great. These were well established families, many were descendants of the first settlers in the area; this wasn't migratory or recent immigrant labor as we have now. So these were families who had been there, many of them longer than any of the other people had been. They were not in business, as they mostly worked in fruit orchards and things like that. Their kids were good athletes and good students, so we got to know them very well in school, and there were no barriers.

Also there was a fair-sized black population in town, because Riverside was a railroad town and the Southern Pacific and Santa Fe went through there. So a lot of these people came in as part of the railroad business. In grammar school and high school there was no separation at all. We were integrated, and blacks went to



the same schools as the rest of us did and got along very well. They were great athletes; we couldn't have done as well in athletics without them. Riverside became a well-known place for athletics and still is.

There was also a fair Korean population. I don't know what their origin was, but they were there. They had nurseries, ran flower shops, worked in yards, and things like that. So we had many Kims and so on, and we also had a few Japanese. So it was sort of a typical southern California society.

### Sports and Social Activities

Hughes: What sports were you playing?

Reeves: I was always interested in sports. My father was interested in tennis, so we had a clay court that we built out on the farm. Tennis games went on there every weekend.

Hughes: With your parents?

Reeves: Well, even when I was just a little child of ten I would run around with a tennis racket. People from town would come out, because we had a court and they didn't have a court. It was just a dirt court, but it was lined, had a net, and we built a chicken-wire fence around it.

Hughes: It seems an unusual thing for your father to do.

Reeves: He was a good tennis player. He was a chop-and-hack man, but he was agile. So we did a lot of that. Then, as we got older, the kids would play a lot. I played on the tennis team in high school, but the city kids didn't know what to do when I showed up, because they all knew each other and who was going to win; they'd grown up playing together on the courts in town. I got interested in basketball, and we had a basketball backboard out on the tennis court. It was the only one in the neighborhood, so a lot of the local farm kids used it.

Hughes: It sounds as though the farm was kind of a social center.

Reeves: Yes, it really was. And there were other families who lived on farms the other side of town. One of them had a reservoir, so we'd go to their place on a Saturday or Sunday to swim in the reservoir. There was a lot of social life.

Also, this was a community where people joined lodges. The lodge my folks belonged to, the Royal Neighbors of America, you never heard of, I'm sure. It was not the Elks Club, the Moose Club, or Masonic Lodge. It was an organization in which there was both a women's and men's side and a lot of social activity. Anyway, it was a nice community to grow up in.

I guess the main thing the people were concerned with in the community was the breaking down of our whole economic system (the Depression) at that stage in history. But the people could live off of what they had on their farms pretty well. They'd eat an awful lot of chicken. I always thought that white butter was the worst thing in the world. If you make butter from cream, it's white. It's not yellow. And I thought if I could just have some of that yellow butter from a store, it must be better. Well, that was just coloring that had been added to it. Of course, Nucoa in those days was just like lard; it was white, and you had to mix the coloring in. You couldn't buy yellow Nucoa at that time; it was illegal because of the dairy lobby.

Hughes: I can remember that.

Reeves: People would come from miles around to our place if they knew we had some butter that my mother would sell. She sold a lot of stuff. Canned a lot of stuff. Paid for my piano lessons by canning for the music teacher. Didn't have the money to pay for lessons. I hated music.

Hughes: So that didn't last very long?

Reeves: Well, it lasted until that happened [shows maimed index finger], and then I had the excuse that it was too painful to practice. [laughing] And it stayed "painful" for years after that.

Hughes: How did that accident happen?

Reeves: One evening my parents went to a neighboring ranch to listen to "Fibber McGee and Molly" on a new Majestic radio. I tackled a kid playing football that evening on the lawn and caught my finger in his belt buckle, got a compound fracture, tore the fingernail out by the roots, and got blood poisoning. That was pre-antibiotics, so I was lucky I didn't lose an arm.

Hughes: Did that ever interfere with your laboratory work?

Reeves: No, that didn't interfere at all. I was left-handed originally, but it didn't bother me much. My mother thought it was wrong to be left-handed, so she forced me to be right-handed. [laughter]

Hughes: Dr. Reeves is referring to the loss of the first joint of the index finger on his left hand.

Reeves: No problem. Lucky it wasn't an arm.

Hughes: Well, tell me, unless there's something that comes first, about high school.

### Early Interest in Entomology

Reeves: I'd rather go to junior high school first, if you don't mind. I'd always been interested in chasing bugs; don't ask me why. Even before I went to junior high school I was collecting butterflies. But I got to junior high school, and there was a biology teacher there. His name was Fred Estes, and he was an avid amateur butterfly collector, and he was a good one. I mean, he did everything right. He was part of a circle of amateur butterfly collectors in southern California. They actually had a club. He found out I had some interest, and he nurtured this very aggressively. I had to mount things right, and I had to label them right. I had to learn to identify them correctly. He had a woodshop, so he worked with me, and I made special cases to keep them in that were nice wooden cases with glass tops and so on, sort of like the museums have. So he really instilled in me some degree of discipline entomologically, and an interest which has held on for a long time (like sixty years).

So even if we go back to high school yearbooks, they always say, "What are you interested in? What are you going to be?" and I always said, "An entomologist." As a matter of fact, the books are right here. If you don't believe me, you can look them up. They all say that I was going to be an entomologist, and everybody thought I was crazy, or they didn't know what the word meant. My interest even led to a nickname in the rural neighborhood of "Billy Bugs Reeves," which didn't bother me any. When I wasn't working, I was usually running around madly in a pair of shorts and pith helmet chasing the little buggers with a butterfly net.

Hughes: Were you thought a bit weird to be doing that?

Reeves: I'm sure in that farming community they thought I was weird, but you know, we were all a little bit weird. No, it was a very friendly weirdness. No one was derogatory about it. They didn't understand why anybody would waste their time chasing insects. But anyway, I think that had a very major influence on me, not knowingly at that time, of course.



Riverside Polytechnical High School, 1930-1934

Reeves: In high school there was good teaching, but there was no teacher there who ever had any influence on me particularly. I mean, I had teachers I respected, but at that time I was getting much more interested in athletics than I was in school, and I got very much involved in basketball, track, and tennis. I was too damn skinny to play football. I always wanted to play football, but I was six feet tall and weighed maybe 135 pounds when soaking wet. The football coach was also usually the basketball coach in those days, so they made decisions on which sport you could play. They didn't want to lose a skinny basketball player who wanted to join the big boys to play football.

Hughes: Were you keeping in touch with Fred Estes during high school?

Reeves: All during high school I did, and then I lost touch with him when I left Riverside. I have no idea, but he's probably not around any more.

Hughes: Was he doing a little more than just showing you the proper techniques?

Reeves: He was a good biology teacher.

Hughes: Was he also encouraging you to go on in the field?

Reeves: Not that I was conscious of. No, he was not an entomologist as such.

Hughes: This was just a hobby?

Reeves: It was a hobby with him, but a serious hobby. Actually, some of the insects that I collected during that time I still had in my collections when I came up to Berkeley, and they were sort of first records for some of these insects in the Riverside area. I was living right there with the bugs.

In high school, I had nothing definite in my mind about what I was going to do except to be an entomologist. I could tolerate the classes that I had to take, but I didn't take shop and all those sorts of things that were offered in a polytechnic high school. A lot of the kids from the farms would take machine or auto shop.

Hughes: So you were taking a solid academic course?

Reeves: I was taking a solid pre-college sort of a curriculum but not realizing what it was.

Hughes: And not with the idea of going to college, necessarily?

Reeves: I thought about that quite a bit. I wanted to do it, but I was really doubtful that I could at that particular time, because we were in the Depression; the mid 1930s wasn't exactly the time when you had a lot of ideas of great things you were going to be able to do. Plus I knew that my family didn't have any money.

### The Depression

Hughes: Did the Depression make much difference to them?

Reeves: Oh, yes. You might have something, but it wasn't worth anything. You couldn't sell it for much. My dad was a beekeeper, and if honey got down to where it was five cents a pound wholesale, something like that, it didn't make any difference. You could have two tons of honey, and it wasn't worth a lot of money. On the other hand, they always were able to save money, because during the twenties I know my dad bought two houses up in the urban part of Riverside. He didn't pay a lot of money for them, but he'd find out about these houses and buy them, and he had them for years, rented out.

Hughes: With the idea of income?

Reeves: Yes. Eventually they had to sell one, and then they moved into the other when they sold the ranch.

So I'd say that high school, as far as I was concerned, was a very interesting time. I wasn't active socially at all, in the sense of going to dances and that sort of thing. I spent my time at the Y. The YMCA had a camp in Catalina which I went to a couple of years, and then I was a leader over there and had a tentful of little kids to take care of and clean up after every morning.

Hughes: Were you still hunting and fishing with your dad?

Reeves: Yes. When the ranch work was done, usually around August, we had an open time when we would take off for about a month. We'd go up above Bishop into the Rock Creek country, or we'd go up the coast

on the Redwood Highway. The Redwood Highway in the 1920s was an interesting place. It was a dirt road, one lane wide in most places, with logging trucks and few tourists. When I was in high school, my parents bought a cabin on the Santa Ana River in the San Bernardino Mountains, and I learned to know every trout in that stream.

Hughes: And you were backpacking?

Reeves: I was backpacking a lot. The YMCA had a High Sierra camping group. We packed into Cedar Grove up in the Kings River country. It was a one-day pack from Sequoia Lake.

### Riverside Junior College, 1934-1936##

Hughes: What made you realize that college was a possibility?

Reeves: Actually, when I went to junior college I ran into a teacher who helped in this regard. His name was Edmund Jaeger, and he was an unusual person to be teaching in a junior college. He had gone to and taught at Pomona College, as I recall. This was a very good liberal arts and science college in southern California. He taught the basic zoology courses at junior college, and he was very serious about it. Basically, he taught a course that was the equivalent of Zoology 1A-1B on the Berkeley campus, which was unusual in a junior college. He was frustrated, because most of the students in the class just couldn't do the work or weren't interested enough to do it.

But he was also interesting as a teacher because he published books on the flora and the fauna of the Mojave Desert and spent a lot of time out there. He took me on field trips to the desert. He had a series of books. I don't have any of those books anymore, so I can't tell you what their titles were. He also had published a book which gave the Greek and Latin derivation of scientific terms. A large part of his teaching in every class was that you had to learn a certain number of these derivations. So you learned the Greek and Latin stems of scientific names, which was very, very helpful later in university courses.

Hughes: Yes, I bet that served you well.

Reeves: I've never heard of an educational system that devoted that much time to that, and I didn't mind it. The only problem that I had was that I learned all the stuff and went to class, and Jaeger soon learned that there were only three or four of us in the class



of twenty-some who knew the material. So he'd just keep calling on us and ignoring the rest of the class, and the rest of the class would get unhappy with this. This same thing can happen in Berkeley today.

The Citrus Experiment Station, University of California, Riverside

Reeves: At that particular time the Riverside campus of the University of California was a citrus experiment station, and it dealt with research on subtropical horticulture. It wasn't a campus in the sense of having any classrooms, and there was no real campus there as there is now. It was a research institution. It dealt with research on citrus and other fruits--walnuts, and so on. There was a really outstanding group of scientists out there.

Hughes: Who were also teaching?

Reeves: They had doctoral students, but they had no classes. It was strictly research where you learned by doing. In the university today, Mount Hamilton, the White Mountain Physiology Research Station, and Bodega Bay Marine Station don't have organized classes, but they're research institutes. Riverside in the 1930s was a big campus in the sense that it had a set of large research buildings, and they had what I would consider even today a sizable faculty of people who had the title "Professor."

The thing that was particularly interesting to me was that biological control of insect pests was a major thing they did. Biological control meant that they first had to find an insect which would be a predator on another insect. A lot of our insect pests have been introduced from other countries with fruits or plants and so on. Scientists go to the country where that insect came from that they want to get rid of. They look there and find some parasite that lives on it that hasn't been introduced with the pest. That's why the pest is doing so well here. So they introduce the predator on this insect. For example, they have control of scale insects and aphids using ladybird beetles, wasps, or whatever. I found that very interesting.

So I started going out to the experiment station and getting to know some of the people. Professor Harry J. Quayle was the head of the station at that time. I managed to get my foot in his door one day, and I said, "I want to work out here." This was during the summer, and I was in junior college. He said, "We don't have any jobs." I said, "I still want to do it. I'll do it for nothing." He said, "Well, we can't do that." There was some

rule against it. After about the third visit out there, he gave up on me. He said, "Okay, you've got a job"--for fifty cents an hour or something like that. So that really put my foot in the door.

I was assigned to a young predoctoral student named Howard McKinsey, and he was working on red and yellow scale, which are very important pests on citrus fruit. They had not been able to tell these two species apart, so his job was to find out how to separate them under the microscope. My job was to spend endless hours mounting specimens of these insects on glass slides. I became a real expert on mounting scale insects on glass slides. McKinsey had been working on this project for a couple of years and hadn't succeeded in separating the species. One day I was looking at all these bugs under a microscope, and I said, "What's this funny little patch of scales that are up in this corner and not on the others?" Well, it turned out to be the way you could separate the two species. So he got a big paper out of that. He didn't acknowledge the fact that I had pointed it out to him, but it saved him, and I learned that credit was beside the point.

I worked out there for two summers. This period had a major influence on my career. They had a bunch of graduate students from the UCLA and Berkeley campuses who spent the summer working in Riverside. So I got a chance to meet graduate students and an undergraduate from UCLA, Deane P. Furman. We wound up being roommates at Berkeley. He was moving up to Berkeley in his senior year, and I from junior college was going for my junior year. He had no place to stay or nobody to stay with, and I didn't either, so we got to know each other and shared an apartment at 1820 Euclid Avenue. We went into medical entomology together, and we're still very close friends. I was best man at his wedding; he was an usher at my wedding. He became professor of medical entomology and head of that program here on the campus in the mid 1940s at the same time that I became a professor in the School of Public Health. He is now retired, still lives here, and we have kept in contact for fifty-four years.

At lunchtime the faculty at Riverside would welcome the students to come and join them. So I got to know people like Professor Harry S. Smith, who was really the father of biological control work, and Harold Compere, who had traveled all over the world collecting parasites and bringing them to California. Al Boyce was there and later became head of the Riverside campus. P. H. Timberlake, who was a very good taxonomist, took me under his wing, and we spent many weekends on trips out in the desert collecting flies and various insects. It was an unusual experience to get to know this group of scientists. With that job, which I just sort of forced myself into, I made contact with



a lot of the people who were the leaders in their field nationally and internationally. That set my mind to go on to college and get a degree and to go into biological control. They were fascinating people, what they did sounded exciting, and I could see who they were and what they were doing.

Hughes: Was it controversial as well?

Reeves: No, biological control, as today, was very acceptable. There were no arguments against it. In today's world, of course, it's the way to go. Well, Al Boyce became one of the world's experts on insecticides; he wasn't in biological control. And there was a very strong toxicology unit there, but that didn't interest me.

Hughes: I would think the toxicologists and the biological control people would be at odds.

Reeves: Not at all. No, they're not in two camps even now. The only time they get into two camps is when the biological control people start getting snotty and say, "You guys don't know what you're doing. You're ruining the world." The biological control people tend to be more do-gooders than the toxicologists do. I'm not being derogatory. But they're not really that separated, even on the campus today.

Basically that set my mind on what I wanted to do. So I went through junior college. I still wasn't a serious student in the sense of spending a lot of time studying. I was very serious about basketball and bugs. We won the championship of our league two years in a row.

Hughes: How were you supporting yourself?

Reeves: I was living at home.

Hughes: There was no tuition?

Reeves: At junior college there's no tuition, and at Berkeley it was very low at that time.

## II UNDERGRADUATE AND GRADUATE STUDENT, UNIVERSITY OF CALIFORNIA, BERKELEY, 1936-1943

### Arrival in Berkeley

Reeves: Well, I decided to come up to Berkeley, and I'd saved a little money from this fifty-cents-an-hour job because I was living at home. My dad said, "I just don't have the money to afford to send you." They could help me just a little bit. I said, "I'll find some way to do it when I get up there. If I don't, I'll come home."

Hughes: Why Berkeley?

Reeves: Berkeley was the only school in the western United States that had a top-rated program in entomology.

Hughes: In the undergraduate years.

Reeves: Yes. There was a very limited program at UCLA and at Davis, but in each of those programs your graduate work had to be finished at Berkeley. So you couldn't finish a doctoral degree in entomology at any of the other campuses in those days. This was in the thirties.

So I came up here, and I was going to be in biological control. Deane Furman hadn't really decided what he was going to do, but we had some interest in medical entomology, so we were taking the courses in bacteriology that were offered on this campus by Dr. Karl F. Meyer, who was the head of the microbiology program on this campus as well as being the director of the George Williams Hooper Foundation for Medical Research on the San Francisco campus.

The first or second year here, we both took the medical microbiology course that Dr. Meyer taught along with Dr. Alfred Krueger. Sandy [Sanford S.] Elberg was the head teaching assistant in that course.<sup>1</sup> There were three teaching assistants: Elberg, I. W. Golub, and Mrs. Stewart. So that was my first time talking with Sandy Elberg in 1938, and I've known him ever since.

Hughes: What was Elberg like as a teacher?

Reeves: Well, he was the teaching assistant in the laboratory, so he didn't give any of the lectures, but he actually handled the laboratory end of the teaching, which in microbiology is really where you spend a very large amount of your time and learn the most. He was Dr. Meyer's right-hand man, and he was called "My Man Friday." The excuse was that the class was given on Friday, but actually, Dr. Meyer treated him just like Robinson Crusoe treated Friday. He'd be giving a lecture and he'd shout, "Friday, where's the eraser?" or "Friday, where's this?" And Elberg would come trotting down and do his job which was to get whatever Dr. Meyer wanted.

To get back on the track of my interest in entomology: I was into this biological control interest, and I was to graduate after two years here. Deane and I had an apartment at 1820 Euclid Avenue, and I think our rent was \$28 a month, something like that. The lady who ran it, Mrs. Watts, let us do the janitor work, and that cut our rent in half.

### Basketball Injury

Reeves: We had an intramural basketball team, and entomology and forestry were the hot contenders for the championship, and we were going to beat them. We had a good team. Tommy Aitken, who is now retired from the Rockefeller Foundation, was a member of the team with me, as was Paul DeBach, who later became a leader in biological control at Riverside, George Ferguson, who became a leader in entomology at Oregon State University, and Ned Bohart, who worked with bees as pollinators of crops in Utah.

Anyway, we were doing great, and then in the middle of the first game in the playoffs I got my foot stuck on the floor and

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<sup>1</sup>Sanford S. Elberg. Graduate Education and Microbiology at the University of California, Berkeley, 1930-1989, Regional Oral History Office, University of California, Berkeley, 1990.

tore the medial collateral ligaments in my left knee. That was the end of basketball. In those days you weren't operated on, because if you were operated on there was a fifty-fifty chance you'd have a stiff leg the rest of your life.

Hughes: How did that affect you?

Reeves: The best thing that ever happened to me, because I could no longer play basketball; I couldn't do anything else, so I started studying.

Hughes: For lack of anything better to do! [laughter]

Reeves: I really started studying for the first time in my life. So a physical impairment can be beneficial. The girl I was going with at Berkeley at the time gave up on me because I couldn't go to dances anymore. I spent a lot of time in Cowell Hospital. They had to practically carry me up here when I had the injury.

Hughes: What did they do for it?

Reeves: They put it in a cast. I was in a cast for a month, and then they took it off, and they said, "Everything seems to be fine. Go out and be active." I got in a softball game the next day and tore it all loose again, so I had to be put in a cast again. I shouldn't have believed them. I still can't believe them, because I've been hospitalized a number of times with it.

Hughes: So it's remained a weak point.

Reeves: Yes, until I broke my leg, and that apparently strengthened it, calcifications and so on. I think having that bad accident probably influenced the development of my scholastic interest more than anything else did. It seems humorous now, but it didn't then.

### Forest Entomology

Reeves: In the 1930s they had an NYA, National Youth Authority, a program you could sign up for which is sort of like the work-study program the students have now. A certain amount of money from somewhere or other comes into somebody's hands, and they can hire students. I got fifty cents an hour to work in forest entomology, a program of the U.S. Department of Agriculture. It was located on the top floor of Hilgard Hall on the campus. Howard McKinsey, the fellow I'd worked for down in Riverside on scale insects, was transferred



up here. He saw me one day on campus and said, "Gee, Bill, wouldn't you like a job mounting scale insects again for me?" He was working on forest scale insects aspects on pine trees. I said, "Yes, great." So I got fifty cents an hour for that, and I supported myself that way. You could live on that sort of money in those days.

That led to my spending two very interesting summers in '38, '39. McKinsey had a forest insect project down in Prescott, Arizona, so he hired me during the summertime to go down there and work on this with him. I got a truck to drive down there, was paid \$125 a month, and I was given a crew of ten to fifteen CCC boys. These were boys in the Civilian Conservation Corps. I used them as my laborers out in the woods, putting *Matsacoccus* scale insects on pine trees and seeing that they killed the tips of the branches, causing dieback. That was a great experience, and it gave me responsibility for leading other people.

There were very interesting boys in the camps. I had one whose first name was Cowboy. He was raised on the King Ranch, and the King Ranch had a policy that if they didn't let the kids get educated they'd stay on the ranch. This boy had never been to school in his life, and he couldn't sign his own name. I got him on my crew and he was a good worker, and it turned out he was bright. I stimulated him; they had a night school at the camp, and he started going to school for the first time. Within three years he graduated from high school. He was a smart kid. We corresponded for a long time.

Hughes: What happened to him?

Reeves: I don't know, but he didn't go back to the King Ranch. [laughs]

But anyway, it was very good to have experience handling a crew of ten or twelve kids who were as old as I was.

Hughes: Wasn't that quite a responsibility to be given to you at that stage?

Reeves: I felt so. It turned out that the person I was working for didn't want to have any responsibility for anything.

Hughes: So it was more that than the fact that he saw great leadership potential in William Reeves?

Reeves: Yes, he just didn't like to work very much. He's dead now, so we can talk about him.

Hughes: What was the result of that study?



Reeves: We proved that the scale insect was causing the tip dieback in the pine trees. They would get on the tips of the limbs out in the young growth, and they would girdle it; so the tips would die back, and then the pine beetles came in on those dead growths and destroyed the trees. I don't know what they finally did about control, because I got out of that after a couple of years. But I had to design the study. For the second time I had the experience that McKinsey wrote up the study but did not acknowledge my participation.

I lived in the CCC camp with the foremen. That was quite an experience. These Forest Service guys were a rough bunch but mostly fine people.

Hughes: Was Prescott, Arizona, a rough place?

Reeves: Yes. A kid in the camp was killed in a racial incident. It was a pretty bad situation.

Hughes: Because he was an outsider?

Reeves: Yes. The city people didn't want the CCC boys to come into their town, or there would be a conflict between boys from Alabama, New York, and Texas who were thrown together in the camp.

The Forest Service was doing hard labor, building trails and this sort of thing. They were good people, but they were rough. This one guy that I was sharing a cabin with got mad at me one day and said he was going to kill me, and he had a gun. I was aggravating him some way or other. Anyway, I finally talked him out of it, but I had to take it seriously; he had a gun. But that's just a part of the experience. I had one kid who pulled a knife on me. Cowboy hit him in the back of the head with a shovel and stopped him.

Hughes: You learned more than just science.

Reeves: The kid got court martialed. Technically, he was under the army. The army ran the discipline and the management of the camps, and the Forest Service did the field training. Anyway, I got \$125 a month and could save enough money to live on the whole next year.

About this time, the idea of biological control had to go down the tubes, because several of the students who were in the Ph.D. program or working down at Riverside came up and either flunked their language exams or their oral exams. So they passed a new rule that you couldn't go to Riverside to start your research until you passed all your exams. I didn't like that, because it

meant I was going to be delayed a couple of years before I could go back to Riverside and do what I thought I wanted to do in research. So I moved into economic entomology with Professor E. O. Essig and didn't know for sure what I was going to do, but I'd probably be doing crop management or something like that. I was interested in general entomology anyway. Meanwhile, I had taken a course in medical entomology and done very well in it.

### Teaching Assistant in Medical Entomology and in Parasitology

Reeves: The interesting thing was that I was going to go into agricultural entomology and had been elected president of the Entomology Club in the department. One day Professor [W. B.] Herms, who was the chairman of the department, came to me. I was just starting graduate work, and he said, "Would you like to have a job?" I said, "What do you mean?" He said, "I'd like to have you be a teaching assistant in the parasitology course and the medical entomology course that we're teaching." He said, "You've been recommended to me by Florence Frost," the instructor in the laboratory. She was his right-hand person. Professor Herms was responsible for the medical entomology course, so he offered me \$45 a month, I think it was. I was a full-time assistant in two undergraduate courses. I never had a pay raise, but it was enough to live on.

Hughes: How much medical entomology did you know?

Reeves: Well, I'd taken the two courses and gotten an A or A+ in them.

Hughes: So you knew quite a bit.

Reeves: They were the only courses on the campus in that area. I'd taken the protozoology and parasitology courses that Professor Harold Kirby was teaching in the zoology department and gotten A's in them, not that A's are necessarily a measure of what you know. I also had taken all the entomology courses that were offered on the campus.

Hughes: You were recommended because you'd not only taken the courses but you'd done well in them?

Reeves: I had a good record, yes. Also on the campus at that time there were lots of activities which brought graduate students from different fields together. In the College of Agriculture there was an organization called Alpha Zeta. That was the Greek

fraternity that was sort of the Phi Beta Kappa of the College of Agriculture. So the best students were invited to be in it. I was elected during my first year as a graduate student, as I remember. There were students in that organization from all the different areas in agriculture.

There also were other organizations on the campus. One of these was a scientific fraternity called Gamma Alpha. Gamma Alpha was made up of graduate students from all the graduate scientific programs on the campus--physics, chemistry, agriculture, botany, microbiology, zoology, etc. You had to be elected to membership, and a lot of people were blackballed and didn't get in. The organization had monthly meetings. We'd wind up at Spenger's Restaurant after the meeting, and you got to know a wide variety of students.

At that time, [Ernest O.] Lawrence and all of his students were very busy building the first [atomic] accelerator on campus,<sup>2</sup> and those people were in Gamma Alpha. We had very interesting speakers. I remember when the first accelerator was built on the campus. We went up there and visited before it even had been put in operation. Then later, when they built the first one up on the hill, we went up there and followed the building process.

There also was Phi Sigma, which was another scientific organization, and Sigma Xi, which is still active on the campus, but I haven't been affiliated with it for a long time. These were honorary societies which had scientific meetings but also had a lot to do with getting to know people on campus and so on.

Hughes: The meetings consisted of a speaker and then discussion afterwards?

Reeves: Yes.

Hughes: Did you ever feel any segregation because you were College of Agriculture rather than Letters & Science?

Reeves: Absolutely not. No, if you were cutting the mustard and these people understood you, there was no problem; you made great friends. A lot of friends were very useful to you at some stage during your research. There'd be something you'd need, some technique or something, and you'd maybe be able to get help from them.

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<sup>2</sup>For more on Ernest Lawrence and his colleagues, see the oral histories in The Bancroft Library with physicists and medical physicists on the Berkeley campus.



Hughes: Did you maintain your ties with the Lawrence group?

Reeves: Not really. But some of those people I'd see around. For instance, Nello Pace in physiology. Nello Pace helped establish the White Mountain high elevation station. I had contact with him for years after that and still do. Lincoln Constance was a member of this group, and people like that. We didn't have a close relationship so that we saw each other all the time socially or otherwise, but we still see each other on the campus. There are a number of people who were graduate students then who are still on the campus. I can't just name them off the top of my head. But basically, being appointed a teaching assistant changed me completely into a different field of activity.

#### Work with the Alameda County Mosquito Abatement District

Reeves: In addition to that, Professor Herms came to me and said, "Do you need some more money?" I said yes, and he said, "The Alameda County Mosquito Abatement District would like to hire a student to work on treehole mosquitoes" (*Aedes sierrensis*, then named *Aedes varipalpus*).

Oak, laurel, cottonwood, and other trees around the Bay Area get rot holes in them, and the rainwater collects in those holes. That's the habitat of the treehole mosquito. This mosquito only lays its eggs in these places, and when it rains the hole fills up with water, the eggs hatch, and then the mosquitoes emerge. Actually, Dr. John Anderson, now in the entomology department, is working on this mosquito as one of his research interests.

The mosquito abatement district had a bad time, because people would call up and complain about mosquitoes bothering them. Their employees would go out and look for water on the ground and try to find the mosquitoes and couldn't find them. Then they'd find those tiny black mosquitoes with white legs, and they knew they were treehole mosquitoes. It was a lot of bother for them to try to find where those mosquitoes were coming from, because you'd have to go around the neighborhood and look at and climb trees, and you had a kink in your neck or could break it if you fell out of the tree. If you saw water running down from a tree up high, then you had to get a ladder or some way to climb up there and see if that's where they were coming from. If it was, then you had to fill the hole with concrete, sand, or something so it wouldn't happen again.



So that was my job. I'd get a call, and they'd say, "Down at Hayward there's a complaint from such-and-such a block. Go down there and see if you can find where the mosquitoes are coming from." It was fun. That actually is where I started my research on mosquitoes, because I got interested in the treehole mosquito, and I established a colony, which hadn't been done before. Every day I put my arm in and let them bite me, so I had a couple hundred mosquitoes biting me a day. I was too stupid to put a rabbit or a guinea pig in the cage, so I put my arm in. Of course, in today's world I wouldn't be allowed to put a rabbit or a guinea pig in there, or I'd be told I'd have to anesthetize it. I wasn't anesthetized, and I did put my arm in.

Hughes: Well, you probably wouldn't be allowed to do that, either.

Reeves: They can't stop you from doing that. Anyway, that led to my first thesis attempt, but we're getting ahead of the game a little bit.

#### Faculty Members, Department of Entomology

Hughes: I think we should go back and pick up on entomology at UCB. Could you please provide some context?

Reeves: Cornell was probably the main competition for Berkeley. Overall, I think Berkeley was considered to be as good as or better than any department of entomology in this country. They had a really excellent faculty. They had Professor Herms as their chairman, who was the leading medical entomologist of the time in the country and who was a very interesting person as well. His doctoral degree was an honorary degree; he had not gotten a Ph.D. as such. Nobody knew that at the time, and he was always called Professor Herms.

Professor E. O. Essig was the head of the economic entomology group, and he was a real leader in that field and had written the textbook in economic entomology. Dr. L. C. Van Dyke was the head of taxonomy, and again a world-known taxonomist working on beetles. Interesting person also. He had no formal training in entomology but was a physician. He'd started entomology as a hobby. He was a pole vaulter in the Olympic Games back in about 1910 or something like that. I think he even won at pole vaulting, nine or ten feet or something. But a leader in the field. Professor William Hoskins was the head of toxicology. Again, he was one of the really leading toxicologists in the country. These were all excellent teachers. Rodney Craig was the

insect physiology teacher, and Guy F. McCloud represented agricultural economics. He later worked out of the President's Agricultural Affairs Office in the university statewide office. There also was a big program on insect vectors of plant diseases, with [Henry D.] Severin and Julie Frietag.

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Reeves: In addition there was a really very capable group of students. There must have been ninety or so undergraduates in that program when I came into it. It was a lot of students, and a lot of those students went on to be leaders in the field of entomology. Then in the graduate program they had a large group of graduate students: Gordon Lindsay, who later was a very important person-- chairman of the Berkeley department and a very well-known entomologist; Robert Usinger went on to head up entomology at Davis; Charlie Michener, who became head of entomology at Kansas State; Ned Sylvester and Ray F. Smith, who were later heads of the entomology department here; Deane Furman in medical entomology, whom I mentioned earlier. There were just endless people I could name who became leaders in the field of entomology here and at other universities.

Hughes: Which faculty members did you have personal contacts with?

Reeves: All of the professors. In those days, before you took your oral examinations for your qualifying examination for the Ph.D., you had to take written examinations in eleven fields of entomology. And you had to have 70 percent or better in each test. You could have one exam where you slipped below 70 and had to be re-examined. If you had two, you were out. You had to cover insect physiology, insect toxicology, economic entomology, medical entomology, apiculture, history of entomology, insect taxonomy, insect morphology, forest entomology and several I have forgotten. We didn't have a course on beekeeping on this campus. The only course on beekeeping was at the Davis campus, so I had a jump on everybody there, as my dad was a beekeeper and I had absorbed more than my share of bee stings. And I knew all the technical terms the beekeepers use. I knew what a hive was, and what a swarm or frame was. Professor [John E.] Eckert, who taught apiculture at Davis, used to come to Riverside and talk and talk to the beekeepers all the time. So I'd met him years ago. He was very pleased when I got 100 on his exam, because I was the first student to do so. [laughs] I never took his course, but I didn't have to.

Hughes: What did your father say?

Reeves: Oh, he thought that was great. But my parents never really understood this business of higher education and exactly what I was doing. It was always sort of a mystery to them. Even worse, when I became a professor at the School of Public Health, my mother could never understand that it wasn't the State Health Department. She just couldn't understand that, as she did volunteer work at the Riverside County Health Department, and all they talked about was the State Health Department.

But basically being in the department of entomology was a real experience, because the students you were surrounded by were stimulating, the seminars were really lively, you had a good faculty, and the faculty cared about you.

Stanley B. Freeborn was a professor in the department and associate dean of the College of Agriculture. He was an unusually effective teacher who taught insect morphology, another one of the fields you had to have in your exam. Freeborn later became the provost or chancellor of the Davis campus. He was the associate dean for agriculture here, and that was when Claude Hutchison was the dean for agriculture. You go to Davis now, and there's Hutchison Avenue and Hall and Freeborn Hall and Haring Hall [C. M. Haring]. These people all moved from Berkeley up to the Davis campus in the 1940s or 50s.

Hughes: Did that hurt entomology here?

Reeves: I'd say that the only way in which it hurt entomology was--and this is one of the things that's always bothered me, and it hasn't changed yet--is that the very best teachers get put into administrative positions. I think this frequently is the case. As a student, I resented it when some of these people whom I was looking forward to taking a course with all of a sudden were moved into administrative jobs and were no longer available as a teacher or on thesis committees. This, I think, is one of the weaknesses of many university systems, not just ours. This sort of business of up or out is something that happens here as well as in government agencies. So many of the very best teachers get put in administrative positions. Elberg, for example, was a very good teacher, and he was made dean of the Graduate Division. I'm not sure which is the most important, administrative activities or teaching. I guess that's not my problem.

Hughes: Well, it was your problem when you were dean.

Reeves: I didn't want to be dean. But we'll get to that later.

Hughes: Was that one of the reasons?



Reeves: Yes, I think it was, consciously or subconsciously. Well, we'll get into that later, because it was a problem with every dean we had in our school until our present dean [Joyce C. Lashof]. All our deans just said they would not leave teaching or research. That doesn't help you on campus. But we had a really outstanding group of people teaching in the early days, and it was very stimulating.

Hughes: Did you have time for anything extracurricular?

Reeves: Well, I still did some fishing, but I didn't have much time for it.

Hughes: Did you get home much?

Reeves: Yes. I wasn't ordered home from Berkeley, but I was certainly told how welcome I'd be at home every summer. As a matter of fact, I had a conflict in this regard, because I'd have opportunities for jobs to make money to live on, and my dad needed me back on the ranch in Riverside to help harvest walnuts or to extract honey and this sort of thing. One of the problems of not having any siblings was that I was the only person who had grown up there, could walk in there and do anything that needed to be done. It was a marginal type of living that they were making, and to hire people to do these jobs just knocked money off the top of income. The way I developed these shoulders and these big breast muscles, which are no damn good to me now, was knocking walnuts out of trees, lifting hundred-pound beehives loaded with honey, or moving and stacking baled hay. You develop a lot of muscles which you don't have much use for later. It goes into flab.

### Mary Jane Moulton

Reeves: I made a point of going home for all the holidays, because I had no reason to stay in Berkeley. I mean, the only home that I had was in Riverside. Plus the fact was that my wife-to-be [Mary Jane Moulton] was living in Riverside, and we went together seriously for several years. She went to UCLA instead of Berkeley until she came to do her graduate work and teaching credentials up here; that helped.

Hughes: You met her in Riverside?

Reeves: I delivered the Saturday Evening Post to her house when I was twelve years old; I had a Saturday Evening Post route, and they were one of my customers. One of the most difficult customers was



her dad, who never wanted to pay me for the magazine. I had to get the money from his wife or from Mary Jane. [laughter] But I never got along with her father that well. No, I'm kidding. We became close friends when I owed him money rather than the other way around.

We went to the same junior high school and were in classes together. In high school, again we were in classes together. But it wasn't until junior college that we became serious and started going together.

Hughes: Was it getting serious by the time you moved to Berkeley?

Reeves: I guess so. [laughter] Yes, we were bothered that we were separated. However, that's something you don't discuss at great length.

Hughes: When did you get married?

Reeves: We got married on July 6, 1940.

Hughes: So you were in the middle of your Ph.D. work?

Reeves: When we got married I was getting \$45 a month as a teaching assistant, and she worked part time in the library or in the department of engineering for \$25 or so a month.

Hughes: Here at the university.

Reeves: Yes.

Hughes: She graduated from UCLA?

Reeves: She graduated from UCLA and got her teaching credentials here. Her teaching experience was in a nursery school someplace here--a miserable experience. Then she went back and taught in grammar school for a year at Riverside.

Hughes: Did she continue after she was married?

Reeves: No, she devoted her time to raising our three sons, Bill, Bob, and Terry, and taking care of me and being a necessary companion.

She has developed a very demanding hobby--competitive swimming in the Masters organization. She goes to local, regional, and national meets and competes in all events. She made All American in the mile. She has swum the Golden Gate Bridge twice, the first time on her seventieth birthday. I'm proud of these activities.

### Research on Treehole Mosquitoes

Reeves: Now, I told you about how I got working on the treehole mosquito. One of the problems in dealing with that mosquito was that the females lay their eggs around the edge of the treehole above the water level. When it rains, the water level comes up, and if the eggs get submerged they hatch out. Well, I had this colony in this cage and I'm feeding them, and I put some cups with water in there and they'd lay eggs around the edge of it so I could get eggs. But I couldn't get the eggs to hatch at the time or in the numbers I wanted, and I didn't know why. When I wanted them for something, I wanted them all to hatch. So I read up on the literature to find out if anybody knew what made this whole group of *Aedes* mosquitoes hatch, which includes our worst pest mosquitoes, snow mosquitoes, and yellow fever and dengue fever virus vectors.

I observed one day in 1940 that if I put distilled water in with the eggs they wouldn't hatch at all, but if I added food to the water, even though they didn't hatch immediately, a couple of days later they would hatch. This was interesting. So I messed around with this and tried to figure out what it was. Then finally I was talking to one of the graduate students in Gamma Alpha who was working in bacteriology, doing a thesis with whooping cough organisms. The discussion led around to the fact that maybe it was bacteria that were causing the eggs to hatch some way or other. So I took some of his cultures of whooping cough, a very unlikely organism to work with, but he was throwing them away, so I took them. I added them to the water, and sure enough, the damn mosquito eggs hatched.

I found out if I did this with almost any bacteria, the eggs would hatch. Then I found if I put the bacteria into a dialysis bag, which is sort of a cellophane bag, so the bacteria couldn't get out but still were taking things out of the water outside the bag, the eggs would hatch. The eggs were outside the bag, so they weren't in contact with the bacteria. Well, to make a long story short, it turned out that what was required was to reduce the oxygen in the water. That was a signal to the mosquito not only that there was water there, but also there was enough food, and it probably was worth hatching.

So that was pretty exciting, because as far as I knew it hadn't been done before. The other group that was working on this was in the U.S. Department of Agriculture up at Portland, Oregon. They were off on a theory that the hatching was due to the

breakdown of leaves and other debris and that amino acids were being released. I duplicated their experiments, and if I did them sterilely it didn't work. So I had the bear by the tail. I had a beautiful thesis problem. I'd worked on it for about a year. I could even put a vacuum pump on the water or bubble nitrogen through it, and that would take the oxygen out and they'd hatch. I was very excited.

### Loss of the Thesis Project

Reeves: In 1940, Professor Herms told us that a gentleman was coming to visit us, name of Dr. F. C. Bishop, who was the head of the Division of Insects Affecting Man and Animals of the U.S. Department of Agriculture. He was one of the big chances for students who had to have jobs, because the federal government was one of the few places that was still hiring people. But I didn't want to talk to him, because I knew that his group was working on the same problem, and they were on the wrong track. In those days a thesis was something you guarded very, very carefully. If anybody else had published it, you couldn't use it as a thesis anymore.

Well, Bishop came, and Herms brought him around to me and said, "Tell Dr. Bishop what you're doing." I didn't want to tell him, and Herms got very angry with me.

Hughes: Why did Herms say that?

Reeves: He didn't know why I didn't want to tell Bishop, because I hadn't told him: "This group's doing the same project." I guess I hadn't had a chance. I hadn't communicated with him enough. Okay, let's put it this way: I was sort of taken by surprise.

Anyway, to make a long story short, pretty soon Herms had told him so much that there was no point in my keeping it a secret anymore, so I got out my protocols. I have the books around someplace. Anyway, he proceeded to copy down my protocols. I didn't like it, but I couldn't do anything about it. My professor was mad at me anyway. So Bishop left, and six months later I was finally given permission by Herms and Freeborn to present my thesis findings to the California Mosquito Control Association that was going to be meeting here that December. But my findings wouldn't be published because they didn't have a proceedings volume that was a publication. "Tell these people what you've been doing, because the Alameda County Mosquito Abatement District has been supporting you," dadadada. "And it must be important to



mosquito control in some way; we don't know how, but it must be important."

So just before the meetings, Mr. Harold F. Gray, manager of the Alameda County Mosquito Abatement District, who had been paying me, called up. He was an engineer. "Bill" he said, "we're going to change the program a little bit. We have a Mr. C. M. Gjullin who's coming down from Portland, Oregon. He's just contacted me by phone, and they have a project they've been working on that they think is pretty exciting, and they'd like to get it on the program if they can. Do you mind?" I said, "No, I can't object. You're the program chairman. I work for you." Well, sure enough, C. M. Gjullin came down and he presented their work, which was my thesis. I didn't know Gjullin at all at that time. However, I became a very good friend of his within a few years.

Hughes: Did he know the story?

Reeves: No, he didn't.

Hughes: What did you do at the meeting?

Reeves: He gave the paper, and I was introduced by Mr. Gray to give the next paper. I just stood up and said, "It's very simple. I don't need to waste your time. Mr. Gjullin just gave my paper for me. Basically it's my thesis, and he's used the same protocol, so there's no need of my presenting it to you." And I sat down.

Hughes: Were the implications clear?

Reeves: Very clear. [laughs] There used to be an auditorium in Wellman Hall. Wellman Hall used to be Agriculture Hall, and the entire center section was an amphitheater, which is the museum now.

So Mr. Gray called a recess. Gjullin came up, and he said, "What do you mean by that?" I said, "Look, here's my paper." He looked at it, and he sort of turned pale. He said, "How can it be?" I said, "I can tell you exactly when you guys got off amino acids onto oxygen. F. C. Bishop visited you," and I told him the date. "Bishop said, 'Have you guys thought about this approach?' You said, 'No.' So he laid out a series of experiments. Am I right?" He said, "Yes, you're right. The paper is already in the press, and if I stop it I will be fired, and I have a family to support.

Well, Herms and Freeborn, needless to say, got on the phone, and they called Bishop and yelled and ranted and raved, and Bishop just laughed at them. He said, "That's life." It was tough. He



had a reputation for doing such things, so they told me, "We're sorry; you can't use this for a thesis anymore"--a year, a year and a half of work down the tubes.

### Colonization of Treehole Mosquitoes

Hughes: Now, let me go back. Why did you decide that you were going to try to colonize mosquitoes?

Reeves: Just because I was working on this mosquito and helping the mosquito abatement district, and they wanted me to do research on the mosquito because it was a very bad pest. It was a real problem, and they wanted to know as much as they could about this mosquito.

Hughes: What did you do that led to success where others had failed?

Reeves: I don't know. I just got enough mosquitoes into a cage and gave them enough blood that they decided this was a good way to live. You know, these things are done sort of incidentally and accidentally.

Hughes: People really had tried to colonize the treehole mosquito?

Reeves: Yes, I believe so. Another student had worked before me and hadn't been able to do it. It's simple now.

### Early Yellow Fever Research

Hughes: Had the mosquitoes in any of the earlier work--I'm thinking of yellow fever--been colonized?

Reeves: The yellow fever mosquito, *Aedes aegypti* (then called *Stegomyia fasciatus*) had been colonized before 1900 by Carlos Finlay, a physician in Havana, Cuba. Actually, the Reed Yellow Fever Commission was sent down by the army after the Spanish-American War to find out what yellow fever was caused by and how it was transmitted. Finlay was a local Cuban doctor who thought mosquitoes were carrying the virus. And nobody believed him. They thought yellow fever was due to a bacterium.

Hughes: Why did he think mosquitoes were the vector?

Reeves: Because he was seeing a lot of cases of yellow fever. Everybody in Cuba who hadn't had the disease got it or was getting it. Any immigrant had almost a 100 percent chance of getting yellow fever. Most American soldiers who were sent down to Cuba got yellow fever, and 25 percent or more of them died. It was a big problem, and Finlay was interested in it because a large part of his medical practice was yellow fever. Finlay finally had gotten into his mind an association between this particular mosquito in the houses and yellow fever. He tried to transmit it by mosquito from person to person and never succeeded. He didn't know that you had to incubate the mosquitoes for eight to ten days at room temperature before they could get it in the salivary glands and transmit it. That's what the Yellow Fever Commission, [Walter] Reed and his associates, worked out.

Hughes: Were they using colonies of mosquitoes?

Reeves: Finlay actually gave the Yellow Fever Commission eggs from this mosquito that he was growing in his house. It was just dumb luck that it was the right mosquito and that the eggs would hatch, because another common mosquito in that situation, *Culex quinquefasciatus*, couldn't even transmit yellow fever.

Hughes: So it really was just luck that Finlay had picked the right mosquito?

Reeves: Well, it was luck plus the fact that it was a very common mosquito in the houses. His intuition or whatever led in the right direction.

Hughes: When was this?

Reeves: Just before 1900.

Hughes: Was anybody else postulating that insects might be transmitting human diseases?

Reeves: M. H. R. Carter, working in Mississippi in the United States, had been doing epidemiological studies on yellow fever and had strong suspicions that mosquitoes were involved but had not done any experimental work to prove it.

The First Consent Form for Research on Human Subjects

Reeves: Incidentally, this document on my wall is the human consent form the Yellow Fever Commission used. It's the first human consent form I know of.

I make my students read the report of the Yellow Fever Commission. It was published in 1900 in the Congressional Record. President Teddy Roosevelt was pushing the work because he wanted to sanitize Cuba. So they did all these experiments, which are reported in detail in the papers that I have the students read each year. They did the research on human subjects because there was no animal host other than man known at that time. It wasn't until the late 1920s that it was found that monkeys could be used.

Well, the students get really up in arms about the Yellow Fever Commission taking these Spaniards and enlisted men and forcing them--well, they didn't force them into anything. I get the students all worked up. They can be the most do-gooding people you ever saw. And they're right. They say, "These experiments never should have been done," dadada. I say, "Well, what would you have to do?" They'd say, "We'd have to have a human consent form." I say, "What would you want to put in it?" They list all the things to be done: You have to be told about your chances of dying. You have to be told if you're going to be paid or not paid. Can you get out of the study if you want to, or do you have to stay in? They make a list of ten or twelve requirements on the blackboard. "Look," I said, "you're forgetting something. Spanish-speaking people are being used, and army volunteers." "Oh, yes, the forms have to be in both Spanish and English."

I get the person who made the most noise in the class, and I say, "Would you read this document [the consent form] out loud to the class?" And of course they can't do it because it's in Spanish. So then I say, "Read the other side, as that is in English."

Everything the students have listed on the board is in the consent form, and as the student reads it, I check the requirements on the board off.

Hughes: Now, whose idea was the consent form? Reed's?

Reeves: I don't know. There's no indication in their publication of a consent form, and there certainly was no law requiring it at that time. The whole thing's there: The chances of their dying, they'll get good medical care if they get sick, how much money

they would get paid, if they died the money would go to a designated beneficiary, etc.

I just discovered this consent form by accident. I didn't know it existed, because the papers never talk about it. I was sitting in the War Room at Walter Reed Army Medical Center, presiding at an army virus commission meeting, and was sort of bored. Anyway, I saw this thing on the wall. I thought, "It looks like Walter Reed." I'd seen a lot of pictures of him. I wondered what it was, called a break, and I went over to read it. I told General Philip K. Russell, "Phil, I need a copy of that for my teaching," and he had that copy made for me.

Hughes: Remarkable.



### III EARLY RESEARCH ON ENCEPHALITIS

#### New Thesis Topic: Research on Mosquito Transmission of Western Equine Encephalitis<sup>1</sup>

Reeves: Anyway, to go back, I was forced to change my thesis. So I have this nice mosquito colony. I know how to hatch the eggs, and I know they'll bite, take blood. In our medical entomology course, western equine encephalitis was one of the topics we talked about. This was a very important disease at that time, because in the 1930s our horsepower in agriculture was literally horses. We didn't have enough tractors or pumps and all the mechanical stuff we have now; we had horses, and that was horsepower. The horses were getting this disease and were dying like flies. I'm talking about thousands of cases in horses, and we didn't know how it was carried. Generally it was thought to be forage poisoning, or it was something being transmitted like influenza from horse to horse. They called it every name in the world, including botulism and Kansas-Nebraska horse plague. But they didn't know what caused it; they didn't know how it was carried. One of the theories was that it was carried by mosquitoes.

Hughes: Do you know who originated that idea?

Reeves: Dr. Meyer, probably as much as anyone, but other people had thought of it, because the disease occurred in the summertime, and there were always plenty of mosquitoes around horses.

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<sup>1</sup>William C. Reeves. *Culex tarsalis* and other mosquitoes as vectors of the virus encephalitides of western North America. Ph.D. thesis, University of California, Berkeley, 1943.

Hughes: I read in Meyer's oral history that he noticed that the cases occurred predominantly in irrigated areas.<sup>1</sup>

Reeves: That's where horses were. Horses had to have a drink of water, and they were in irrigated areas where crops were grown. But I'm not taking any credit away from Dr. Meyer at all. We'll get around to him and more of what he contributed in just a moment.

So I had this colony of mosquitoes, and I decided, why don't I see if they can transmit the viruses? I'll fill in the background that leads up to that in my thinking. But there was no course in virology on the Berkeley campus. Currently, there are such courses in molecular biology, public health, microbiology, and plant pathology; there are courses all over the place. But in the 1930s there was no course in virology. It was a new field.

#### Karl F. Meyer's Isolation of Western Equine Encephalitis Virus<sup>2</sup>

Reeves: The one place at the university that was working on viruses was Dr. Meyer's group at the Hooper Foundation [at the San Francisco Medical Center]. Plus he'd lectured on this disease in our microbiology course. It wasn't a course on virology, but he included this, and we talked about it. So I went over to Dr. Meyer--he knew who I was--and I said I wanted to try to do transmission experiments, but I didn't have any particular knowledge of how to do them; but I knew that Miss Beatrice F. Howitt worked in his lab. I said, "Is it possible that she could teach me how to work with these viruses?" He said, "Yes, great idea." So that's what got me in that group.

Hughes: Were they working with the western equine virus?

Reeves: Yes, with two viruses, western equine and St. Louis encephalitis.

Now, you've read in Dr. Meyer's history, I'm sure, about how he made the original isolation of the western equine

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<sup>1</sup>Karl F. Meyer. Medical Research and Public Health, Regional Oral History Office, University of California, Berkeley, 1976, p. 218.

<sup>2</sup>For better chronology, this and the following five sections were moved from their original position later in the same interview.

encephalomyelitis<sup>1</sup> virus from horse brain. Other people had tried to do this and had not succeeded. He put out the word that he was interested in finding young horses that had just come down with the typical disease, and he wanted to actually go himself to get the brain material and get it back to the laboratory in good condition.

So he got a call concerning this typical case that was in a young animal down in Merced County.<sup>2</sup> At that time there was a veterinary group that worked here on the campus, and it included Drs. C. M. Haring and Jake Traub. There was not a veterinary school at Davis then; this was back in the thirties. So there was a group in Berkeley that was working on brucellosis, on various diseases of chickens, and so on. They had a chicken farm up in Strawberry Canyon.

Dr. Haring, who was a veterinarian, and Dr. Meyer went down to the Central Valley. They got to this farm, and it was a typical case. I'll just quickly review the history, although it is told in a film that was made of Dr. Meyer in a lecture at Walter Reed Institute for Medical Research.<sup>3</sup> They filmed him doing his lecture. He's the only person on the screen the whole time.

The case was just what they wanted, so they wanted to sacrifice the horse; it wasn't dead yet. In those days of the Depression a horse wasn't worth much. The farmer said, "No, you can't sacrifice the animal. That's a very valuable horse," and dadadadadada. Dr. Meyer offered him \$50, and the guy said no. So what can you do? The guy didn't want his horse killed, so they were out of luck. But as he's leaving, the wife catches Dr. Meyer and says, "That horse isn't worth \$50 to me. Give me the money." He said, "Well, how am I going to get it?" Meanwhile, the farmer stomped off; he's mad or something. She said, "Look. You come out here tonight after dark. Turn your car lights off, and you sit out there. When I put a lamp in the window, my husband's asleep, and I guarantee you I've taken care of him. He's not

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<sup>1</sup>The virus was originally described as western equine encephalomyelitis virus, but subsequent viruses were named encephalitis viruses; so this term will be used for simplicity.

<sup>2</sup>See Dr. Meyer's oral history, pp. 215-217, for his version of this story.

<sup>3</sup>A. S. Benninson at California State University, San Diego, transcribed the movie onto videotape. He and the American Society of Tropical Medicine have the videotape.



going to wake up. When that lamp comes on in the window, you go ahead and kill the horse and get his head off and get out of here." So Dr. Meyer gave her the money.

That night they sat out there and they sat out there, and finally the lamp came on in the window. Over the fence they went, knocked the horse out, and chopped his head off. Over the fence, threw the horse head in the back of the car, and took off for town. They drove up to this all-night gas station where there was light, because they wanted to get the brain out and pack it in ice. The guy comes out all excited. He had a customer, you know, and in those days on Highway 99 at midnight there were not many customers. They dragged this horse head out of the back of the car, and this guy didn't know what to do. Anyway, they took the brain out and packed it in ice. By daybreak the next morning they had gotten back to San Francisco and had a brain suspension inoculated into rabbits and guinea pigs, and they isolated the virus.

Hughes: Was that the first isolation?

Reeves: The first. I tried for years to get the name of that person, the farm, and the address, because I wanted to find out what the hell the story was from their viewpoint. Dr. Meyer would never give it to me. He never would.

Hughes: He didn't want an irate farmer down his neck.

Reeves: Well, this was ten, fifteen years later, but Dr. Meyer wouldn't budge. He wasn't going to tell me where to go.

#### Further Research on Mosquito Transmission

Reeves: Meanwhile, General R. A. Kelser at the Army Medical School in Washington, D.C., had a colony of the yellow fever mosquito, his *Aedes aegypti*, which isn't related to *Aedes sierrensis* that I was working with but looks a little bit like it. He knew the yellow fever mosquito could transmit yellow fever. He fed his mosquitoes on infected animals that had the western equine virus in their blood and transmitted it from guinea pig to guinea pig, and from guinea pig to horse. However, the mosquito he worked with didn't even occur where the disease was. I mean, *Aedes egypti* never had occurred in California. So it was sort of a nonsense experiment, but it showed mosquitoes could transmit it. I was as bad as Kelser: I went ahead with *Aedes sierrensis* and actually showed I could infect the species by feeding it on infected guinea pigs,



but I could not transmit the virus. I did transmit WEE [western equine encephalitis] virus with *Aedes dorsalis* (now known to be *Aedes melanimon*) in 1941.

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Hughes: Transmitted the infection to what, now?

Reeves: To guinea pigs. I didn't have any horses, and I wasn't going to feed the mosquitoes on me. As a matter of fact, at that time we didn't know that humans were actually infected with this virus. But Dr. Meyer suspected it. Some veterinarians associated with horse cases had gotten an illness very similar to encephalitis, but they couldn't prove what it was.

Well, Miss Howitt was very excited about the work I was doing, because now she had an entomologist as a captive. So she wanted me to go to Kern County in the summer of 1941 and collect mosquitoes, and she'd isolate virus from them if she could. I thought that was great, even though I didn't know if she would pay my way or not. So I was all set to go to Kern County. Meanwhile, I was identifying mosquitoes collected from Kern County. Her work was concentrated in Kern County, because they had a very cooperative health department and lots of cases of central nervous system disease down there--polio and encephalitis. She had now proven that humans were getting western and St. Louis encephalitis in that area, as well as the horses getting western.

The entomology department here in Berkeley was the only source of people to identify mosquitoes in California. The State Health Department didn't have anybody who was able to do that. Tommy Aitken and I were doing a service for mosquito abatement districts by helping them learn to identify their mosquitoes. He and I were having special sessions with mosquito control people, teaching them how to identify mosquitoes with a microscope. We also arranged a service so they could ship mosquitoes to us from their districts if they had trouble, and we would identify them and give them back the results. So I was getting mosquitoes in for identification from the Dr. Morris Mosquito Abatement District in Kern County, and I knew what was going on there.

This whole thing ties together with Dr. Quayle, whom I'd gone to in 1935 at Riverside to get a job.

Hughes: I remember.

Reeves: Well, Dr. Quayle had done the first mosquito survey of California in 1910 and had developed a control program for salt marsh mosquitoes in Burlingame. At that time he was here in Berkeley.

Then Herms had come here in the early 1900s, and he and Freeborn had organized further mosquito surveys of California. This is the Freeborn who I told you was a professor and later went on to administration at Davis. In 1926 he wrote the first book on the mosquitoes of California, which I have over here on a shelf.<sup>1</sup>

Hughes: That's the one which says nothing about viral diseases.

Reeves: He talks about malaria some. When he wrote that book in 1926, they didn't know mosquitoes were vectors of encephalitis viruses.

All these things tie together. The big statewide mosquito surveys were done during the WPA [Work Progress Administration] days when they had a lot of extra money to hire people and carry out control digging of drain ditches with a shovel. They'd go out and make collections and dig big ditches to drain swamps and so on. And there was a huge collection of mosquitoes packed away in Ag Hall.

#### Mosquito Control Programs in California

Hughes: Was all this being done primarily because of the disease factor?

Reeves: No, I have to go back one step. Actually, the reason that the mosquito control program was started in California, and the reason that Herms became very involved, was that malaria was a highly endemic disease in the Sacramento Valley, in the Mother Lode country, and all the way down into the San Joaquin Valley. Bakersfield was limited in its development because malaria was so highly endemic. The first malaria-mosquito control work in an endemic area in the United States was organized in California at Penryn in the foothills of the Sierras, and it was developed by Herms and [Harold F.] Gray solely to control malaria, which was a really important disease. In the days of the forty-niners it was a big problem. The Dr. Morris Mosquito Abatement District was formed in Bakersfield in 1917 to control malaria.

Hughes: So malaria was the impetus?

Reeves: That was a real impetus.

Hughes: The annoyance factor of mosquitoes was secondary?

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<sup>1</sup>S.B. Freeborn. The Mosquitos of California. Univ. Calif. Publ. Entomol. 1926, 3:333-460.

Reeves: Right. In the Bay Area there was no mosquito-borne disease, but the salt marsh mosquitoes were serious pests. The bay area along Emeryville, and where Spenger's Restaurant is now, was all a big salt marsh. There was salt marsh all around this bay. The mosquitoes that came out of those salt marshes were so bad that real estate development in this area was slowed down and even stopped. And that's when they developed the mosquito control programs to drain the marshes, and then real estate development took place.

In the 1930s, when I came up here, we were still supposedly getting invaded in Berkeley by salt marsh mosquitoes from Marin County. The Alameda County Mosquito Abatement District marked the mosquitoes in Marin County with a dust that I developed in the 1940s to mark our mosquitoes. Ted Aarons, their entomologist, dusted and released them and recovered them in Berkeley within twenty-four hours. So all this research was tied together in a cooperative project by nine Bay Area mosquito abatement districts.

Harold F. Gray was the manager of the Alameda County Mosquito Abatement District, but he also taught environmental health in our School of Public Health, originally the Department of Hygiene. He was an engineer, and he had appointments in the Department of Hygiene and in the School of Engineering on the Berkeley campus. He and Herms wrote the first book on mosquito control.<sup>1</sup> There are two editions, 1940 and 1944. These two persons worked very closely together, and Harold Gray actually got into the business working with Herms on malaria in California. He was the only engineer, I think, who has ever been the Health Officer of California at a time when only physicians could be.

Hughes: What were people thinking about when the terms arthropod-borne viruses or arboviruses were first used?

Reeves: We first called them mosquito-borne virus encephalitides in the early 1940s, when that relationship was discovered.

Hughes: Why don't you use the early 1940s, when you were entering the field, as the time frame?

Reeves: In 1941 I'd been doing this work with Howitt. I was scheduled to go to Kern County for her. Meanwhile, Dr. Meyer had decided that he wanted to develop further a research unit at the Hooper

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<sup>1</sup>W.B. Herms and H.F. Gray. Mosquito Control: Practical Methods for Abatement of Disease Vectors and Pests. New York: The Commonwealth Fund, 1940 and 1944.



Foundation to work on central nervous system virus diseases. These were viruses that affected the central nervous system, which would include polio and encephalitis viruses--western, St. Louis, or anything else that showed up. But those were the three diseases that he was most concerned about.

#### Recruitment of William McDowell Hammon

Reeves: Dr. Meyer had been traveling around the country, trying to find some young physician to bring in to head up this research unit. He went to Harvard and visited Hans Zinsser, who was a very close friend of his and a very, very famous microbiologist. Zinsser wrote a famous book, Rats, Lice, and History.<sup>1</sup> Dr. Meyer told him what he wanted to do, and Zinsser said, "I've got the guy for you. You want Bill Hammon." Bill Hammon was Zinsser's last teaching assistant. He had been a Belgian Congo medical missionary before he'd gone to medical school. He had been ordained as a minister originally. As he was going to the Belgian Congo as a missionary, they sent him first to the Belgian School of Tropical Medicine to learn enough tropical diseases so he could do the medical end of the missionary work. He'd been in the Belgian Congo four years and found that he could not do what he needed to do medically, so he came back to Pennsylvania, finished his pre-med stuff at Alleghany College, went to medical school at Harvard Medical School, and was going to go back to the Belgian Congo. Then World War II started, and the Belgians wouldn't let any Americans go back to the Belgian Congo, so that's when Zinsser said, "Look, why don't you do the Doctor of Public Health degree with me. I'd like to have you as my teaching assistant."

Bill Hammon had worked on staphylococcal toxins and Boston cream pies as a source of food poisoning, and he had developed the kitten test to detect toxins. He developed the first vaccine for cat panleukopenia, a virus disease which is a common disease of cats. He made a vaccine for it that's still used today. But he'd also worked on polio with a very active group of people at Harvard interested in poliomyelitis. Zinsser said, "You want the guy who has worked on one of these viruses? I've got him here. He's well trained." So Dr. Meyer hired him and said, "Okay, pack up your family." Bill had two kids, a wife, and a grandmother who lived with them. He had to pack up all these people and come West.

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<sup>1</sup>Boston: Little, Brown and Company, 1934.



Hammon was heading this way in July 1940, and he was going to stop at the Rocky Mountain Laboratory of the National Institute of Allergy and Infectious Diseases in Hamilton, Montana, because they were doing some work on encephalitis viruses. Meanwhile, Dr. Meyer had gotten a call from the Washington State Health Department saying, "We need help. Send somebody up here. We have an epidemic of encephalitis in the Yakima Valley." So Meyer got hold of Hammon on the phone and said, "Don't come down. Send your family down; you go to the Yakima Valley and do an investigation." So Bill got things started up there that first year. There was both western and St. Louis virus there in a major epidemic, as well as the horse disease.

In '41 he's organizing a team to go back up to the Yakima Valley, and he's looking around because he wants an entomologist. He finds the only entomology student in the whole bloody United States who's interested in these viruses, knows anything about them, and knows something about mosquitoes is sitting over here in Berkeley. So he wants me, and I say, "I'm sorry, I've made an agreement with Miss Howitt to go to Bakersfield. I can't go to Yakima." Dr. Meyer says, "The hell you do." [laughter] "You're going to Yakima, Reeves. I forbid you to go to Bakersfield with Miss Howitt." He was trying to remove her at this stage, and one of the reasons he brought Hammon was to replace her.

Hughes: Why?

Reeves: Because she was a very independent lady. At times she was very, very difficult to work with, and she was always getting mad about something and slipping a resignation letter under his door. He decided this time to accept it.

Hughes: Two strong personalities. He was hardly a milquetoast.

Reeves: Anyway, I was released from my responsibility to Miss Howitt, and I was told to go to Yakima. A wonderful friendship and productivity with Dr. Hammon was started for me. I completed my thesis while on that project.

The Doctoral Thesis and Qualifying Exams##

Reeves: The original thesis committee had Professor Herms as the chairman, and Dr. Hammon and Dr. Meyer. But Professor Herms had been taken off by the army, reinstated as a colonel in the medical corps, and was off doing training programs in medical entomology for the army. So I had to have a replacement as the chairman, and they made Dr. M. A. Stewart chairman. He was a professor at Davis and had selected me to be his teaching assistant in the first course in helminthology on the campus. He was the only person left in the university on any of the campuses who was in medical entomology at that time. He came on the committee in 1943 when the thesis was well along its way, and the work really was all done; I was writing it up.

Hughes: What fields did you cover in the qualifying exams?

Reeves: I will have to pull on my memory on that, because that's a long time ago, and I may not even remember all the people who were on the committee. Dr. Stewart was the chairman of the committee, and I had to take medical entomology as one topic. I took parasitology as the second topic, and both Dr. Stewart and Dr. Harold Kirby were on the committee--Harold Kirby from the zoology department, who was in charge of teaching parasitology there. I had Dr. Rodney Craig, who represented the field of insect physiology. I originally had been going to have Freeborn on for insect morphology, but he was not available, so they had to change my committee and put Dr. Julie Freitag on for that field, which was an interesting move. I'd known him as a graduate student, and he'd recently gone on the faculty. He was interested in insects as vectors of plant viruses, and morphology was not his field. I think I had four changes on my committee from the time it was appointed to the time that I sat down for the examination.

I also had Dr. Hugh Cameron, who was a veterinarian from the Davis campus who was interested in virology. He was a Canadian, and I'd never met him until the day of the examination, which was an interesting procedure. I'd never had a course from him. I'd also never had a course from Dr. Freitag or Dr. Stewart.

The last person that was on the committee was Dr. S. F. Light from the zoology department. In those days they had an interesting procedure where the dean of the graduate division would appoint a person on a committee to represent the graduate division, what they called a "wild man." It was his privilege to ask anything he wanted. And again, I'd never had a course from

Dr. Light. He had a reputation for being a very difficult man, which indeed he was.

Cameron in some ways was covering the field of microbiology, which was another one of my topics, and I think that's the last field that I had to have at that time. We had five fields that we were covering. Today they only allow you to have three fields. He was a virologist, and it turned out he liked to hear a lot about horse encephalitis, so he and I had a good time at the examination. I could talk about horse encephalitis forever, as you know.

So that was my committee. The amazing thing was that during the war you never knew who was going to be on a committee. It made no difference if you'd ever had a course from that individual; all you were doing was trying to cover the necessary fields. Originally Dr. Meyer was to be on my committee in microbiology, and I forget why he wasn't on it. I'm probably lucky that he wasn't, because he had a very bad reputation on examinations. But I don't think he would have buffaloed me.

Hughes: How did the examination go?

Reeves: It went for three hours, and I was wringing wet when it was through. It must have gone well, because I passed.

Dr. Craig was sick and couldn't be at the examination, so when the examination was over they couldn't tell me how I'd done until I'd been examined by him, which I did about two weeks later at his home one night. He was a very good friend of mine and an excellent teacher in physiology. I went to his house, and we sat there for an hour and a half, I guess, talking about everything--his illness and what I was doing research-wise. His wife came home after we'd been sitting there an hour and a half. She was a physician who had been out on call. She says, "How did Bill do on his exam?" He said, "What exam?" [laughter] It was an interesting process, but apparently he was satisfied during that time, because he told his wife, "I decided long ago that he'd passed the exam." That was very nice.

Hughes: You just chatted?

Reeves: We chatted, and we had a nice, friendly visit. He continued to be a friend for many years.

Hughes: Were the other examiners pretty thorough?

Reeves: I thought so, but I think the candidate never knows how thorough they've been. All he cares about is what that next question is



going to be, and can he handle it. Dr. Light was very kind. He started asking me what factors in the environment affect the activity of biological agents. And of course I had oxygen, and I had humidity, and I'd gone through a whole list of things, and he finally said, "There's one important one that you haven't covered." I thought about it. He says, "Gosh, it's getting warm in here. Would you like to take your coat off?" I said, "Yes, it is warm in here. Thank you very much." I took my coat off, and we sat there looking at each other. He says, "If you're hot, why don't you go open the window?" I finally said, "Temperature! Is that what you want?" [laughter] It took two hints before I could come out with "temperature."

I mean, examinations are very difficult. I've always been very sympathetic to students ever since that experience. I think students always think that some of the examiners are unfair. I felt that way. I talked to Dr. Kirby about what I was to cover, and he said, "There's one thing that I won't ask you, and that's to derive the phylogeny, the order of development, of all the different protozoa. I know I have a reputation for asking that question, but I won't ask you that." Fortunately, I didn't believe him. The first question was to go to the board and develop the phylogeny of all the protozoa.

Dr. Freitag assured me that one thing he would never ask me was to explain the arrangement of the intestinal tract of a leafhopper, because it's a very involved thing. They suck up sap, and they have to get rid of the water in order to concentrate the sugars and the nutriments. So they have a little bypass pump that takes all the water out. It's a very involved thing. He said, "I won't ask you that because I don't expect anybody other than a plant disease man working with insects to know that." The first question he asked me was to go to the board and draw the leafhopper's intestinal tract.

I don't know; I guess it was just a game. If I had said I didn't know it, they probably wouldn't have cared. In those days it was almost the rule that when you came up for that examination, you should have read every reprint that the examiners had ever written, because everybody seemed to be on an ego trip. They always wanted to ask you to talk to them about what they'd done.

Hughes: And that was true in your case?

Reeves: It was certainly true in this examination. Everyone asked me about their own work except for Cameron. He wanted to talk about horse encephalitis because he was working on poultry diseases and knew nothing about horse encephalitis, although it turned out he knew quite a bit.



I've had a rule ever since then to never ask a student to know everything I've written or even to have read any paper I've ever written. If they want to talk about encephalitis, that's fine with me; I can talk about that. But if they want to talk about something else, that's also fine. I want them to pick the disease, and then I'll ask them about its epidemiology or something like that. That seems to me to be fair. If they pick the disease and then can't handle it, then that's their problem. [laughs]

Hughes: It seems to me that your thesis was far broader than the average thesis and set the pace for a field that was going to unfold rapidly in the future.

Reeves: I think you're probably right that most theses are very focused on some aspect of a problem and not on the entire problem. I think what you have to realize is that when I wrote that thesis in 1943 I'd been involved personally and very deeply in all aspects of the work that was going on. If I wasn't doing it myself, I was supervising it with somebody else--the Yakima work, the virus transmission experiments, the various studies in various places. In the thesis, I covered not just Yakima but also things that were done in Arizona, in New Mexico, and the Texas survey as well. So we had all those studies that had been done in 1941, '42, and '43.

I'd been involved in collecting the material, I'd been involved in doing the laboratory work, and it was a mutual agreement that I would be able to write my thesis on any aspect of the work that had been done, because most of that had never been published before. We still had this barrier that if things had been published before, they couldn't be the basic core of your thesis. So I couldn't use just the original isolation of virus from mosquitoes; we'd already published that in Science. I couldn't use the fact that I'd been successful in transmitting virus by a certain species of mosquito alone, because some of that had been published, but not all of it. So what the thesis had was a lot of additional detail about things that were not covered in publications, because most of our publications had been short, immediate notes about what we had done. So actually it wasn't really until 1943 and later that we put together at various national meetings the overall view of some of the things we'd done.

The result was I had studies that were new and fresh; I had techniques that were new; I had an opportunity given to me to interpret what these findings meant, which we hadn't done yet. I think much of the educating of Dr. Hammon about insects took place while I was writing the thesis. Some of these things we hadn't

even talked about, and I was able to weave them into the thesis. A person who's on the thesis committee you would think would be pretty obligated to read it.

Now, in those days, we didn't have word processors. We had ditto machines or onionskin copies at the best, which didn't give very good copies, as we typed everything. The copy that I have of my thesis is still an onionskin copy, because it's never been put in any other format than that. The original thesis, which was put in the biology library, is the only one on bond paper. Any time somebody on the thesis committee wanted a change to be made, that was tough.

Well, Dr. Stewart prided himself on being a real tiger on correct language. He edited until hell froze over. I guess I was in about the third draft of the thesis when he still hadn't signed it. This was in 1943, and I was due to go to Kern County in May that year for the first Kern County project. The end of the semester was coming up, the filing date was coming up, Hammon had read the thesis and he was very happy with it; everything was fine, and he was a good writer. But Stewart was still fussing around.

One day Dr. Meyer stopped me in the hall. I'd given him his copy, as I had a copy for each person. He said, "Reeves, what's happened to your thesis?" I said, "You have it, Dr. Meyer. You'll have to tell me what happened." "Well," he said, "Is it a good thesis?" I said, "Yes, it's a good thesis. At least I think so. Unless you judge it isn't." He said, "I've been very busy. Has Hammon read it?" I said, "Yes." He said, "I have to talk to him. You stay here. I'll come back." He came back in a while, and he said, "Hammon says it's all right. Why don't we sign it, and it's all over?"

I said, "Dr. Meyer, I'm having a little difficulty with Dr. Morris Stewart," who actually had been doing work for Dr. Meyer. Dr. Meyer had him on his payroll part of the time, so he looked on Stewart as a newcomer and a flunky. He said, "Where's your title page? Do you have it with you?" I said, "Yes." He said, "Give it to me." He hadn't read my thesis at that stage. He did later, and complimented me. He signed it on the top line, where the chairman of the thesis committee always signed. He walked into Hammon's office, and he said, "Sign this." And he said, "Reeves, take this over to Stewart and tell him to sign it." I said, "Dr. Meyer, that's not easy." He said, "I'll call him."

So I went to Berkeley with my title page in my hand, wondering what was going to hit the ceiling. Dr. Stewart hit the

ceiling. He gave me hell for having Dr. Meyer sign where he was supposed to sign--that was the important thing. I said, "Dr. Stewart, I'm not here to argue with you. I'm just doing what Dr. Meyer told me to do. I have a real problem. I have to leave next week for Kern County. That's a job, and I can't give it up. I've made all the changes you've asked me. If you consider that you can sign it after I've gone, would you mind filing it at the Graduate Division?" So he blew up again and he said, "I could never do that for a student." I said, "Then what are we going to do?" He said, "I don't like it, but I'll sign it, and you file it." He'd read every page of the thing repetitively.

Anyway, writing the thesis was fun. I had an unusual opportunity to come back to the original questions and to cover experimental and field work, to cover the different situations in different places--Texas, Yakima Valley, Arizona, New Mexico--and an opportunity to digest and to interpret, which was a very challenging thing to do and very stimulating. Fortunately, some of the findings stood the test of time--which is very satisfying.

#### Virology in the Early 1940s##

Hughes: Before you delve into that, please say something about the general state of virology at that time, both in terms of the concept of the virus and also what techniques were available.

Reeves: It was very primitive. The interesting thing is that on this campus the first measurement of a virus had just been done before or at this time by Professors T.R. Rawlings and Takahashi down in the plant pathology department. They had done it by light refraction through tobacco mosaic viruses and some way or other had worked out what size they were.

Hughes: What about [William J.] Elford and his collodion membranes to measure viral size?

Reeves: I don't know about that. But anyway, supposedly the people in our plant pathology department did the first measurement by light refraction of the particle size of tobacco mosaic virus.

It was very confusing at that time, because a lot of things were being called viruses that we now know are not viruses, and things were being called rickettsiae or something that we now know are viruses. Our techniques were extremely primitive. We were just learning what animal hosts were susceptible. We certainly



had learned that you couldn't culture these things in any media at that time.

Hughes: What did you think a virus was?

Reeves: It was some little thing that you couldn't see, you couldn't smell, but it was there and it was infectious.

Hughes: And it couldn't be cultured?

Reeves: It could not be cultured on artificial media, which is still true today. Instead of artificial media we use live cells as a culture medium.

Hughes: It was filterable, right?

Reeves: We could filter it, but our filters were very, very primitive. They were Berkefeld filters, which are clay filters. You knew that you could get filters that the virus couldn't go through. You'd get those that it would go through, and the bacteria wouldn't go through. We weren't very sophisticated in what size range that really was. But Berkefeld filters were the only method we had, and they were sort of pillars of clay that you put stuff in and put a vacuum on and pulled the stuff through. This preceded collodion membranes, the ultracentrifuge, and electron microscopes by some years.

Hughes: If an infectious agent went through a Berkefeld filter, was it therefore a virus?

Reeves: We called it a virus. If it went through a Berkefeld filter that was calibrated at this size, it was a virus or a toxin, because it wasn't a bacterium, and it wouldn't grow in an artificial medium.

Hughes: What about the agent of psittacosis?

Reeves: No, that wouldn't go through.

Hughes: Yet the chlamydia were confused with viruses.

Reeves: They were called viruses, yes. But they would go through some filters smaller than what bacteria would go through.

Now, the other thing was, we had to make sure they weren't toxins. Because, you see, a toxin would go through a filter, and a toxin would make an animal sick. But the thing about it is that toxins wouldn't multiply when they were passed, so if you did serial passage in a series of animals, that meant you had done serial dilutions and it had still multiplied, and that excluded a



toxin or a chemical. But basically, the methodology was very iffy. Pathology was used extensively because you got more or less typical pathology with a virus infection that you wouldn't get with the other agents.

Hughes: Do you remember hearing about Wendell Stanley's work? Before he came here, he crystallized tobacco mosaic virus.

Reeves: Stanley's stuff on tobacco mosaic virus had not been done at this time. It was done, I think, after this.

Hughes: No, he crystallized tobacco mosaic virus in 1935.<sup>1</sup> There's no reason that you should remember, because that was before you were interested in virology.

Reeves: I knew Wendell very well after he had received the Nobel Prize and come to Berkeley. Obviously we did not appreciate the importance of his work in 1940, and it was not discussed in our course in medical bacteriology.

To be candid, when we learned of Wendell's work on tobacco mosaic virus, we felt it didn't solve any of our problems with disease in horses or people. In those days, comparison between plant and animal viruses wasn't even a gleam in anybody's eye.

Hughes: They were thought to be two different games?

Reeves: Well, they were two different ballparks.

Hughes: In the case of the Rockefeller Institute, the two groups were geographically separated. The people working on animal viruses were at the Rockefeller Institute in New York, and people like Stanley, working on plant viruses, were out at Princeton.

Reeves: The people working on the arboviruses included Max Theiler, who was at the Rockefeller Foundation. It wasn't the Rockefeller Institute, but they were in the same building. So all those people who worked on arboviruses as they evolved were there, too, but they didn't work with Stanley at all. Dr. Richard E. Shope, who was working on swine influenza virus and who was the first one to show the virus's life cycle through pig lung worms and earthworms, was at Princeton with Stanley. Incidentally, his son is Dr. Robert E. Shope, who is now the director of the Yale Arbovirus Unit, where Wil Downs was. He was trained by Wil Downs,

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<sup>1</sup>W. M. Stanley. Isolation of a crystalline protein possessing the properties of tobacco-mosaic virus. Science 1955, 81:644-645.

Max Theiler, Jordi Casals and Otis and Calista Causey of the Rockefeller Foundation.

Hughes: Now, what about techniques?

Reeves: We came in just at the right time. One of the real problems with viruses was how you could maintain them without doing constant serial passage in animal hosts. You had to have some way of preserving these things. Well, we used glycerin to preserve them, and that wasn't very good. But fortunately, about this time dry ice came in. Dry ice really was not developed for this reason but as a refrigerant for frozen foods. But it gave us extremely low temperatures--minus 70 degrees Celsius. It had been found out about that time that if you sealed viruses up in glass ampoules and put them in dry ice, they would maintain their viability for long periods of time. So this really was a major breakthrough in technique.

Hughes: Do you know who was responsible for that technique?

Reeves: I really don't. I know that I was the first to really make dry ice usable as a mosquito attractant, but I don't know who did the other work, and I don't know who would know. It was a major breakthrough, because we could go out in the field and go 300 or 5,000 miles away. If there was dry ice there we could get specimens, preserve them, and ship them home. But when we went to Okinawa in 1945 we were stuck with no dry ice. It wasn't until liquid nitrogen became available much later and REVCO freezers that we could operate without dry ice.

Hughes: The military couldn't supply it in Okinawa?

Reeves: No. When there's a war on, dry ice isn't a high-priority item. We tried to preserve collections in wet ice with salt, which didn't preserve things worth a damn. We were really handicapped.

Hughes: Wasn't that the reason that K. F. Meyer was so much concerned about personal involvement in the field? A lot of the material on horse encephalitis virus was sent to him by veterinarians--I guess they were in the field--but by the time the material got to him it was useless.

Reeves: It was useless, yes. The virus was inactive. That's why he went out himself to get the stuff, pack it in wet ice, get it back, and get it quickly into serial passage in animals. Then he was stuck with the virus once it was isolated; he had to keep passing it or it was lost. He had no way to preserve it.

Hughes: Did he have immense animal colonies?

Reeves: Yes. The Hooper Foundation had originally been built as an animal facility for a veterinary school that was at the San Francisco Medical Center. There was a veterinary training program at San Francisco, and the old Hooper Foundation building was its headquarters, and the animal quarters were all built in there. There was a whole series of big wooden barns up on the hill behind the medical center at the edge of Sutro Forest. All our animals were kept in them. In today's world, they'd close it yesterday, as it met no standards for animal care!

### Retrieving the Hooper Foundation Monkeys

Reeves: Well, to show you the extremes of how bad it was, we were working with monkeys. We had hundreds of monkeys, because that was our only host for polio research. One day an animal caretaker really screwed up, and the next thing we knew, we had forty to fifty monkeys loose in Sutro Forest.

Hughes: [laughs] What did you do about that?

Reeves: Dr. Meyer said, "Reeves, get them back." [laughter] He always solved these problems the easy way.

Hughes: And what did Reeves do?

Reeves: Well, I got an awful lot of bananas, because I figured they'd get hungry, and put the bananas in cages. A lot of the animals came right back to get fed, and we got them into their cages. However, there were still some animals loose up in Sutro Forest that wouldn't come back. The ones we had locked up were safe, so I got a shotgun, went up and shot some of them. The police objected, and then a couple of the monkeys headed downtown to Market Street.

Each day the phone would ring, and Meyer would say, "Reeves, your monkey [laughter] is down on Market Street." We'd go down there, and the monkey would be sitting up on a telephone pole. I've got a shotgun in the car, but the cops are there, and they won't let me shoot the monkey, and I know it was inoculated with polio virus, which could create a panic if the public knew it. There's nobody can get a monkey off a telephone pole if he doesn't want to come down, and he wouldn't come down, and he just wouldn't come to the bait. There were two monkeys originally, and we got one of them some way or another.



One day our head animal caretaker at Hooper, Paul Valdez, went home, and when Paul went into his apartment on Market Street, the damn monkey was sitting on his kitchen table. Paul closed the window. Now, why did that monkey ever go to Paul Valdez's apartment? It probably smelled like monkeys to him, because you never could get rid of that smell.

Hughes: Did the story get into the newspapers?

Reeves: Well, they knew what we were doing, sure. I think the cops tipped them off, but the whole story never came out. Where were we? We were preserving a virus, and now we've gotten into monkey business.

#### Funding after Dr. Hammon's Departure<sup>1</sup>##

Hughes: You mentioned that Dr. Hammon left for Pittsburgh in 1949. We didn't talk about the fact that he took the research contracts with him. Did that leave the group high and dry?

Reeves: Well, it did leave us high and dry, as he was the director of the research and in charge of the epidemiology teaching at Berkeley. It left us high and dry in the sense that he took the polio research and funding with him. But I really wasn't that concerned about polio, and it was proper that he took that with him; it was all his concept. He took the work on Japanese B encephalitis with him, which I had not been deeply involved in and which he was still following up on in Japan. He took Dr. Al Rudnick and Gladys Sather with him because Rudnick was still working with him on the Japanese B encephalitis in Japan and actually went out there after he got to Pittsburgh. Gladys Sather was his head laboratory technician, and he took her with him. He invited me to go along, but I had no interest in going at that time.

So he took the army money with him, and he took the polio money with him. But he left the small National Institutes of Health grant with me, which we had had for only a year at that time. That project was designed for work to be done in California and in Kern County. It was a small one, a total of \$15,440. He also left the staff that I had in Bakersfield and the rest of the laboratory staff with me. So basically I wasn't left high and dry, but I wasn't loaded with money.

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<sup>1</sup>For better chronology, this section was moved from the transcript of interview 5.



Also, at that time we were still getting money from the state health department on and off for very specific projects. These monies actually had started in 1946, when the state legislature earmarked funds for research on encephalitis control, the biology of the mosquito vectors, and things of that type. So basically my position and research were not dependent on Hammon's grants. I had a faculty appointment, as I was an associate professor of epidemiology and never had gone through being an assistant professor at Berkeley.

Hughes: Why was that?

Reeves: I'd gone up the scale in the research series and had lecturer appointments at San Francisco, and I'd also been appointed lecturer in the school at Berkeley until I finished my master's degree in 1949. The dean, Dr. E. S. Rogers, had the bright idea that if I was going to be on the faculty, which was apparently settled at that time, I also ought to have a degree in public health. I didn't want a degree in public health particularly, but I took the M.P.H. degree. I guess I started taking it with the first class in the school in 1945, and it took me three or four years part time to get enough courses to finish the degree.

Hughes: You were doing all this in your "spare time"?

Reeves: Yes, this was all spare time. Plus going to army meetings and state health department meetings.

But Hammon didn't leave me high and dry in any sense of the word. As a matter of fact, I think I've told you enough about Dr. Hammon for you to know that he would have considered it extremely unethical to have left me high and dry.

Hughes: What he took made sense, and what he left made sense.

Reeves: What he took was his most intense interests, and he never did return to do detailed work on the encephalitis viruses that occurred in California.

It's like when Dr. Meyer brought Hammon and me into the program and developed that unit: he never did any more work with encephalitis viruses, and he never infringed at all upon what we were doing. He was supportive, he assisted in interpretation of some of our results, he was interested in what we were doing, but he never again directed what we were doing, how, or why.

Hughes: Why do you think Dr. Meyer stayed out of it?

Reeves: I think he was too busy doing other things, and I think if the people he brought in were doing a good job, he saw no reason to be involved. He did this in several areas.

Karl F. Meyer##

Hughes: I'd like to hear your impressions of K. F. Meyer.

Reeves: I don't know how far I want to go on this one. He's got a lot of admirers still around, including me. Well, Dr. Meyer was a really amazing individual, there's just no question. He was one of a kind; I thought of him as a genius. He was a most unusual person. I'm sure Sandy Elberg said a lot about K. F., because he also knew him very well. He was a man who made really major contributions to our knowledge of the largest array of diseases. I don't know any individual who ever studied as effectively and did more good for industry, for humanity, for science than he did on a wide variety of diseases.

Just look at what he worked on. He saved the cannery industry from a botulism problem, and for practical purposes he did it alone. I'm sure Sandy talked about that in his review. Fish poisoning: the fish industry was in real trouble with fish spoiling on boats and so on, and he developed a detecting system that determined the degree to which they were spoiled and shouldn't be used for food, et cetera. Mussel poisoning was a major problem on the Pacific Coast. A poisonous plankton in the ocean was ingested and concentrated by the mussels, and this poison is one of the most dangerous ones that we know today. His studies led to the quarantine each summer along the Pacific coast to prevent eating of mussels.

Then you look at brucellosis. Dr. Meyer saved the dairy industry by the development of vaccines and skin test antigens that allowed the immunization of susceptible animals and sorting out infected from uninfected animals. Prior methods had always been to go in and slaughter all the animals that were positive or well in infected herds. All the valuable stock was being destroyed, the genetic pool was being destroyed, and he stopped that. He was the world's expert on psittacosis, "parrot fever" or ornithosis, in domestic birds and wild birds. He's the person who wrote the book on this problem in the United States. He's the one who solved the problem when the World Fair on Treasure Island had a big problem with parrots and the people working around them and possibly tourists getting the infection during the World Fair.

Hughes: Why were there parrots on Treasure Island?

Reeves: For an exhibit for the World's Fair. They were shipped in from Australia and elsewhere, and they were loaded with the infection when they got here. He's the one who saved the whole parrot industry by developing a method of feeding parrot colonies on antibiotics so that they could be certified as being psittacosis free. He also was deeply involved with psittacosis as a major problem in the turkey industry. He was a world's expert on plague and worked out the basic cycles of infection in small rodents. You could go on and on and on and on naming diseases. This man had his hand on everything.

Hughes: How do you explain it?

Reeves: He just had an unusual mind, energy, and dedication. He had a photographic memory for references. You could sit with him, talking about something, and he'd say, "Have you read the article about So-and-so in Such-and-such a journal, such-and-such a year, such-and-such a volume, and such-and-such a page number?" And you'd say, "No." "Well, go look at it." You'd go, and that's where it was. Only one time I ever knew that he ever got caught wrong on one of these, and that was by one of the people (Fern French) who worked with us, and she caught him when she was a student. He made the mistake of being two pages off on a reference, and he never forgave her for telling him in front of the class.

Hughes: Did he have a photographic memory in general?

Reeves: If not, it wasn't obvious. Nothing in science that he ever knew or read had been forgotten, and his knowledge of the literature was worldwide, as was his experience. It was like having a computer that had all of these things stored away, but it also had a mind working. It wasn't like a computer, where you had to push the right button to get the answer out. His ability to associate things was absolutely amazing. He had an intuition for associating things. The only other person I've known in my career who I felt was his equal in this regard was Sir MacFarlane Burnet in Australia, and I think that's why they were such good friends. They respected each other so much. The only difference was, Mac Burnet got the Nobel Prize, and K. F. never got equal recognition.

Hughes: Do you think K. F. was ever nominated?

Reeves: I don't think so, because they don't give Nobel Prizes for breadth. It's for a discovery. When Burnet got his, he told me, "I almost didn't accept it, because I didn't think that's what I should get it for."



Hughes: What did he get it for?

Reeves: He got it for his studies on mechanisms of immunity, and he thought he should have had it for developing the embryonated chicken egg as a method and medium to grow a wide range of infectious organisms, including influenza. I knew him well enough that he talked with me when I worked and traveled with him in Australia in the 1950s.

### Meyer as a Teacher

Reeves: But you asked me to tell you more about Dr. Meyer. The other thing is that he was an absolutely unbelievable teacher. He was a teacher of unusual ability because he could also have had a career as an actor. He could captivate an audience, he had an infallible memory, and he really could ham it up. I'll give you an example.

One day we were in the microbiology course that he and Dr. Al Krueger taught. Dr. Meyer was spending most of his time in San Francisco, so you never knew when he was going to come to give a lecture. We'd all be in the lab working and be right in the middle of some critical experiment, and he'd come in and say it was time for a lecture.

Hughes: There were no second thoughts about it?

Reeves: There were no second thoughts about it. He was there, and now it was lecture time. So everybody had to fold up whatever experiment they were doing, maybe go back and start over again a week later. And it might be at two o'clock, it might be at four o'clock, it might be at five o'clock he'd come in. Then you'd go into the classroom, and the class was supposed to be an hour or so in length. Four hours later, he's still talking, and he's just getting warmed up. Of course, students who had jobs had to get up and leave at six o'clock. They'd get up, and he'd go, "Yayayayaya," at them all the way out the door. It would scare the hell out of them. I always found it fascinating; I could sit and listen to him as long as he wanted to talk.

Hughes: Was it anecdotal?

Reeves: Some was anecdotal, but mostly it was fact. I mean, bing, bing, bing, bing, bing. I'll give you another example of the ultimate of this. One day he came in. There was going to be a laboratory demonstration, so we all had to gather in the front of the lab.



There were all these lab benches where we were working. He comes in, and he's got his white coat and rubber gloves on, everything's right. Sandy Elberg and company come in with this cart with this white sheet over it, and there were some dead rabbits under it.

So Dr. Meyer says, [Dr. Reeves adopts German accent] "Today I want to demonstrate to you da pathology of plague." They put a dead rabbit out on a tray, and he takes a scalpel out, opens the rabbit up, reaches in with one gloved hand, takes out the spleen or liver, I forget which it was. He turns to a student and says, "Vas is dat?" The student wasn't sure what it was. "Das is spleen. See dose white dots? What's dat? Dose are lesions. You don't know the pathology, you don't know the disease. I'll have to illustrate this to you."

So he slams this organ back in the animal--and he's got this bloody hand--turns around to the blackboard behind him, and there's a projection screen to roll up. He grabs the cord, jerks it down, lets it loose: Psssst! All this done with a bloody hand. He picks up the eraser and starts erasing the board and throws the eraser down and picks up the chalk. Meanwhile, here's Elberg and company with buckets of Lysol running around and picking up the eraser, putting it in the Lysol, picking up the chalk as soon as Meyer drops it. All the students were pulling back further and further. They know all about plague; he's already given a big lecture about pneumonic plague.

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Reeves: Then Meyer says, "What, you think I'm dumb? That's not plague! That's coliform organisms. Just looks like plague!" [nervous laughter] Well, nobody who ever experienced that lecture would ever forget it. You also didn't forget what a spleen looked like, you didn't forget what a lesion looked like, you didn't forget anything. Then he would actually have an animal there that was infected with plague, but then he was damn careful what he did.

Hughes: Was he available at the end of lectures for questions?

Reeves: Not really. Everybody was so tired by the time he got through, they went home. I don't remember ever having long discussions in the classroom. There wasn't much left to be said.

Also, he prided himself on being a pathologist. The famous story about him was when he first came to the United States and was on the faculty in Pennsylvania at the veterinary school. He was at a veterinary conference someplace, and the word came that this elephant had died at the zoo. They wound up deciding the elephant ought to be autopsied. K. F. made bets with people--

he was, wearing a tuxedo--that he would go get any tissue from that elephant that anybody wanted without getting his tuxedo dirty. He had all sorts of bets, and he did it and collected on all the bets.

Another aspect of Dr. Meyer that I should mention was his pride in having an honorary M.D. degree. Relatively few people knew he was a D.V.M. [Doctor of Veterinary Medicine], as he used the honorary M.D. and was accepted as such at the medical center.

I don't know how much he talks in his oral history about his time in South Africa with [Sir Arnold] Theiler and the fact that they were far from friends.<sup>1</sup> There's a detailed book on Theiler<sup>2</sup> that doesn't mince words about his relationship with Meyer. This book on Theiler's life in Africa follows his whole career there and how Meyer and he didn't last together very long. It leaves no question that Theiler was the man in charge.

Hughes: Is this the Theiler who's the father of Max Theiler of the Rockefeller Foundation, who won the Nobel Prize for development of the yellow fever vaccine?

Reeves: Yes, Max Theiler is the son of Arnold, and that may be why K. F. Meyer and Max Theiler never got along famously. It was almost a family feud that never ended.

Hughes: How was it, being a young person starting out in science and working under Dr. Meyer?

Reeves: Let me tell you how it was, starting out. My only knowledge of Dr. Meyer when I first went in to see him and wanted to get into research on encephalitis was that I'd been a student in his class, and I'd been a good student in that class.

Hughes: He knew you?

Reeves: He knew who I was, perhaps because of contacts when he talked to the local chapter of Delta Omega. Delta Omega was a national honorary society in public health. We didn't have a school of public health but did have a department of hygiene. Some way or other, they'd gotten approval for a chapter of Delta Omega in the Department of Hygiene here. So we had another one of these

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<sup>1</sup>See: Karl F. Meyer, Medical Research and Public Health, Regional Oral History Office, University of California, Berkeley, 1976, pp. 42-43.

<sup>2</sup>Thelma Gutsch, There was a Man: The Life and Times of Sir Arnold Theiler K.C.M.G. of Onderstepoort. Publ. Howard Timmins, Capetown, 1979.

organizations that had monthly or bimonthly meetings at someone's home. Professor Herms of the entomology department had become very much involved in the Delta Omega operation because of his medical entomology interests, and the people in the Department of Hygiene sort of swept the medical entomology people in with them because of their common interests. I was elected to membership with other graduate students in medical entomology and parasitology. We had a lot of evening meetings at people's homes, where Dr. Meyer often came and talked. Some way or other, from classes or Delta Omega, Dr. Meyer knew who I was. I never found it at all difficult to make contact with him at any time.

When you got to the Hooper Foundation, you had to realize that he was the boss, and that was it. He wasn't a particularly patient boss, but he had ways in which he more than made up for this. Let me give you an example. Dr. Meyer never drove an automobile in his life, so someone always was his driver, and he would change drivers periodically, for whatever reason.

#### Driver for Meyer

Hughes: Was the driver under his own employ, or did the university provide him?

Reeves: He had a university car that was at the Hooper all the time just for him, but he didn't drive it. Some employee had to drive him wherever he wanted to go, whenever he wanted to go. I was fortunate; I was one of the people that he picked. For about two years I was K. F.'s driver. Now, this can be a hell of an inconvenience, because if you're right in the middle of inoculating a hundred or more of something, and Dr. Meyer says, "Reeves, I want to go to the Family Club," you don't want to go to the Family Club; you want to finish doing the mice. But he's ready to go, and you go.

You learn to have some extra clothes around the place, because he not only was going to the Family Club and you were going to drive him, but you also were going to go into the very exclusive Family Club with him. Now, you maybe don't know what the Family Club is.

Hughes: I know a little bit about it, but I've never darkened the door.

Reeves: Well, you can't, because you wear a skirt, and that's not permitted in the club. One of my first experiences at the Family Club was when Meyer said, "We're going to the Family Club, Reeves;



Sir MacFarlane Burnet is in town." You do a quick change of clothes, because you had to wear a coat and a necktie in the Family Club, and you go in and you have lunch with Sir MacFarlane Burnet. On other occasions, Hans Zinsser. Paul de Kruif came, the famous writer.

Hughes: You mean de Kruif who wrote Microbe Hunters?

Reeves: Yes, Paul de Kruif the writer.

Another day Dr. Meyer came in with Basil O'Connor, FDR's lawyer and the head of the National Foundation for Infantile Paralysis. And you know, it just went on and on like that. Now, it wasn't that you just were a driver who took him down to the club or to a good restaurant and then went and sat in the car and waited. You went in the club with him, had a fabulous meal, and conversed with these people about their interests and your research.

Hughes: Did he do that because he knew that would be a good experience for a young person?

Reeves: He never told me. But he sure as hell knew it wasn't going to hurt you.

Hughes: He could have had you wait out in the corridor or even in the car.

Reeves: He wasn't that thoughtless. People thought he was, but he really wasn't. I got to know Dr. Meyer awfully well because I traveled with him, and he would tell me stories by the hour. We went up one time to Hamilton, Montana, in the middle of winter for a conference, and he wouldn't stay in Hamilton, because in his opinion there wasn't a decent hotel there, so he had to stay in Missoula. Everybody else was staying in Hamilton. So I had to drive him back to the Missoula hotel every night, and I had to stay in the Missoula hotel so I'd be available the next morning. It was icy and snowing. God, I'd never driven on ice in my life, and they gave me an old station wagon to drive. But we made it, and I sure heard a lot of stories.

Hughes: Was it a monologue?

Reeves: It was a monologue. We'd get to the hotel and he'd say, "I haven't finished the story yet. Come to my room." And he'd maybe open a bottle of cognac and talk until two in the morning.

Hughes: What happened when you were talking about a research project?

Reeves: This was an interesting thing. When Hammon formed the research unit at the Hooper Foundation, my fear was that Dr. Meyer was going to be constantly badgering us about things and be in our way. He never did. When he brought Hammon in, he completely withdrew from any further involvement himself in encephalitis or polio research. That didn't mean he wasn't interested, but he never badgered or was critical about progress. He always was a willing listener any time that you wanted to talk to him about what you'd done and was helpful on interpretations or ideas. He was very helpful on details of pathology when it came up, because none of us were up to his standards as far as pathology was concerned. He was as supportive as anybody could be. I'll give you an extreme example of support.

#### Meyer and Polio Transmission by Flies

Reeves: I guess it was in 1948 that the National Foundation got very upset because a fellow by the name of James Watts at the Public Health Service had done a study on enteric diseases and the degree to which flies were involved in their spread in the lower Rio Grande Valley of Texas. He had isolated polio virus out of flies. The National Foundation got very upset because it had the attitude, "This is our area, and nobody steps into our territory."

I'm sitting at the Hooper minding my own business, when Harry Weaver, I think it was, who was then the medical director for the National Foundation, suddenly showed up at the Hooper and walked into my office. I didn't know this gentleman. My office wasn't very private, as the partitions only went about that high, and everything was open; it was an old barn. He came in, sat down. "Bill," he says, "you have a change of careers." I said, "I do?" He said, "Yes, we've made a decision that the National Foundation is going to get into flies and polio in a big way, and you're going to be in charge."

Well, the National Foundation was supporting most of our work on encephalitis, because the Public Health Service and the army weren't that much involved at this stage in financing. The National Foundation was interested because polio and encephalitis still were confused both diagnostically and epidemiologically. He went on, and he said, "Now, you're to really get to work on the flies and polio business and put Watts out of business. If you have anybody you want on your staff in the whole United States as an entomologist, virologist, or whatever, just tell us who they are and where they are, and we'll get them for you." I said,

"Whether they want to come or not?" He said, "Yes. I'll just go to whomever is responsible for them, and I'll buy them out."

Well, he went on this way for a half an hour or so, and finally I just said, "Doctor, you may be enthusiastic about this, but I think this is a dead end, and I'm not interested. I'm very happy with what I'm doing. I think it's important what I'm doing, and I won't do it." Dr. Weaver said, "Well, wawawawa; I'll get you fired." I said, "If Dr. Meyer fires me for this, okay, I'm fired, but you get Dr. Meyer to come and tell me that I have to go."

Hughes: You thought the fly route was wrong?

Reeves: I really thought there was nothing in it because of my epidemiological knowledge and my intuition, plus the fact that we already had tested about fifty thousand flies collected during polio epidemics in Kern County and gotten nothing out of them.

Hughes: You were in the middle of the Bakersfield field study?

Reeves: Yes, but polio was a side issue for me at that time. The main thing we were working on was mosquitoes and viruses. In 1943 we did our first project in Kern County, and by 1944 I was collecting flies, because a study had come out in Science from Dr. John Paul's lab at Yale, reporting the first polio isolations from unidentified flies. By '45 I'd done enough flies that I didn't want to see one again.

Hughes: You knew they weren't carrying polio virus?

Reeves: I couldn't find it, and I'd inoculated a lot of monkeys with ground up flies.

Hughes: Why had Weaver and the National Foundation gotten onto the fly thing?

Reeves: They knew what was being done. It was being published in journals by the Yale and Public Health Service groups, and they couldn't have somebody doing it who wasn't under the National Foundation. As far as they were concerned, they had a monopoly and would freeze out the Public Health Service and everybody else. They had more money than the Public Health Service had for such research, and they wanted a monopoly. The same attitude prevailed later in the development and testing of polio vaccines.

Anyway, I said, "You get Dr. Meyer to tell me I'm finished, and then I'll have to make a decision about what I'm going to do. But I know what it's going to be; I'm going to say no." So Weaver



went in to see Meyer. Dr. Meyer's office was a glass cubicle about the size of this room. You could hear anything that went on in that room if you were on the top floor of the building, because Dr. Meyer sounded like a foghorn when he talked, and if your ear was tuned in, you could hear it.

Weaver said, "I've been talking to Reeves. I want him to move to polio and flies and he won't do it." Dr. Meyer said, "Really?" Weaver said, "I want you to tell him he has to." Dr. Meyer said, "What did Reeves say?" "He said he wouldn't do it." "What are you talking to me for?" said Dr. Meyer. "He said he wouldn't do it; he won't do it. I know him." Weaver said, "I want you to make him do it." Dr. Meyer said, "Why should I?" Weaver says, "Because I need him for this." Meyer said, "Well, if he says no, I say no." Weaver said, "Well, we'll remove all your money for polio research." Dr. Meyer said, "What? You sit there for five minutes, and let's see if that's true." He picked up the phone and called O'Connor and said, "This guy Weaver's in here telling me he's going to take all the money away from us for polio. Is that right?" He said to Weaver, "O'Connor said no." [laughter] End of conversation. Well, Weaver never forgave me for that. And it wasn't too many years after that that they pulled almost all the support out of our polio program.

Hughes: You think there was a connection?

Reeves: I know there was a connection, but it was the best thing that ever happened to us, because about that time we were getting ample support from the Public Health Service and the army.

#### **Meyer as an Administrator.**

Reeves: Dr. Meyer was a very unusual person. If he needed it, he knew where the money was. If he really needed something, he could call the canners association, he could call the fisheries group, or many other people. If he needed money for some reason he could get it, but not necessarily big money.

At the same time, he was such a tight administrator. I worked at the Hooper all during the war for \$125 a month and didn't get raised to \$150 until after the war was over. Then I didn't get a raise until after they hired Walter Mack from Michigan to work on polio and paid him more than they were paying me and put him under me. I went to Dr. Meyer and said, "You know, Dr. Meyer, there's something wrong with this system. I'm sorry; I'm not complaining, as I'm very happy. But you have a person

under me whom you just brought in who is in getting more than I'm getting. You'd better put him in charge of me." Dr. Meyer replied, "Oh, no, you're in charge." "Well, then maybe I ought to get at least as much as he's getting." He said, "I have to think about that." The next day he said, "You've got it." However, he didn't give me any more than Mack was getting.

Dr. Meyer had a very peculiar method of administration that I've never seen anyone else use. He knew that we all knew about it, so it was no secret. When someone really screwed up, he almost never said anything directly to that person's face about the problem. His method was to be sure that the responsible guy was in the room, and then he'd get somebody in that room who was completely innocent and give them holy hell for what this other person had screwed up on. He'd just go on, shaking his finger at you and screaming at you. Eventually you learned what he was doing and why, but it was very confusing at first.

Hughes: I should think so.

Reeves: I came back from Bakersfield one time in 1944. I'd been there for some months without being at Hooper at all. I was sitting on a dry ice box, telling Bill Hammon what was going on in Bakersfield, as I'd just come back. Dr. Meyer walked in that room and up to me. He yelled at me. He said, "Reeves, if you ever leave a mouse room in a mess again like you left it this time, I'll do this to you and do that to you," and he just gave me hell. I hadn't been there for two months.

Well, I recognized immediately what was going on, because over there in the corner washing some dishes was the person who'd left the mouse room a mess. Dr. Meyer knew who'd done it, but he wouldn't yell at him because he might hit him or something. I don't know why. So Dr. Meyer got all through, and I said, "Yes sir, I'll clean it up right away." I went over to clean up the mouse room, and sure enough, mice and sawdust all over the floor. I was cleaning it up, and Hammon comes in. He's very confused by all this, too. "You didn't do this; you've been in Bakersfield." I said, "I didn't do this, of course not." "Well, what are you cleaning it up for?" "Dr. Meyer told me to clean it up." About this time the responsible person shows up. "Come on," he says, "get out, it's my mess. I'll clean it up." Hammon says to me, "Dr. Meyer can't talk to you that way. You work for me." "That's your problem," I said. "I've learned to survive in this situation."

So Bill went over, and in his very polite, missionary way told Dr. Meyer, "You should not talk to Bill Reeves that way. I should talk to him if he's done something wrong." Meyer said,

"Have you talked to Reeves about it?" Hammon said, "Yes."  
 Dr. Meyer said, "Is he worried?" Hammon said, "No." "Well, then,  
 what are you worried about?" [laughter] And that was his way of  
 administration.

I came back another time and all the ladies had quit who  
 worked in the kitchen. The dishes and lab equipment weren't  
 getting washed; we couldn't go on doing lab work. I hadn't been  
 there, but he calls me into the office. Miss Beatrice Eddy is  
 there, and his secretary Mrs. Rankin is there. He says, "Reeves,  
 what are you going to do about this mess we've got in the dishes  
 room?" I said, "Anything you want, sir." He says, "That's the  
 trouble with everybody's attitude around here," and he gave me  
 holy hell. I didn't even know the details, but he in fact was  
 giving Miss Eddy hell. She knew it. So it was an interesting  
 experience, but I always felt that when things came to push or  
 shove, I didn't have to worry; he would support me.

This was all during World War II, so I developed "The Royal  
 Order of the Purple Heart." It was given to anyone who was blamed  
 for something he hadn't done. A cloth heart stained with Giemsa  
 was pinned on a lab coat. Dr. Meyer knew about it, as one day he  
 asked me if it really hurt.

He was completely ruthless in oral examinations for the Ph.D.  
 He broke down student after student. If they cried or froze up he  
 said, "Well, you're not ready yet." It was very rough. I don't  
 think any woman ever passed an oral exam if he was on the  
 committee.

Hughes: And yet he had two top women, Dr. Eddy and Miss Howitt.

Reeves: Yes, they were very important people. They were both good basic  
 scientists.

He was supportive of many important issues. When we get into  
 the organization of the School of Public Health, another aspect of  
 Dr. Meyer will come out. He was the lobbyist behind the whole  
 movement at the Sacramento end. We wouldn't have a school of  
 public health if it weren't for K. F. Meyer.



Discovering New Species of California Mosquitoes##

[Interview 2: December 6, 1990]

Hughes: Dr. Reeves, last time we talked about the aborted thesis project, but we didn't cover the earlier and perhaps simultaneous work on mosquitoes. Could you tell me about that?

Reeves: Actually, I gave you some indication of my beginning interest in mosquitoes with the project on the treehole mosquito. At the same time that was going on, I had an interest in mosquitoes in general. It is sort of a side issue, but the Department of Entomology at the University of California, Berkeley, had a large collection of mosquitoes that had been collected all the way back to the early 1900s. They had done statewide surveys on mosquitoes. This collection had never really been taken care of and never had been used as a source of information. So in 1938, in order to learn to identify the mosquitoes of California, I started working my way through this endless collection of mosquitoes and identifying them, getting records of where they came from, and so on.

To my amazement, I found four specimens of a species of mosquito that had never been recorded in California. They'd been collected way back in the 1917-1919 period. It was a genus of mosquitoes called *Mansonia* that was not known to occur in California at the time, and the species was *Mansonia perturbans*, which has now been changed to *Coquilleltidia perturbans*.

So here's a specimen of this mosquito from Bakersfield, Kern County; here are two specimens from Galt and Holt in San Joaquin county; one from Landers, Placer County. There were four specimens, and they had been misidentified as *Culex tarsalis*, which I wasn't caring about at that time. It was very exciting to find a genus and a species of mosquito new to California that wasn't even listed in Freeborn's book, The Mosquitoes of California. I wrote my first scientific publication on this new finding, a one-half-page note in the Pan-Pacific Entomologist in 1941.<sup>1</sup>

Hughes: You were looking at this collection under the microscope?

Reeves: Under the dissecting microscope. I wanted to learn to identify all the mosquitoes that occurred in California, and one way to do

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<sup>1</sup>William C. Reeves. The mosquito genus *Mansonia* Blanchard in California. *Pan-Pacific Entomologist* 1941, 17:28.

it was to go through this collection methodically and make sure that the mosquitoes were correctly labeled and correctly identified. We also developed a distribution map of each species in California as an output of this study. This was just a service sort of a thing for the department. So it was an interesting by-product to find a new species.

At about the same time, I made a trip in 1940 down to Riverside, California, on a holiday. In those days anytime I went anywhere, I'd collect mosquitoes. I was interested in treehole mosquitoes because of my other project. One day in the Santa Ana river bottom below Riverside, I saw this huge cottonwood tree, and I could see water running down the trunk, which meant there was a source of water up above someplace. There obviously was a big hole in the trunk of the tree. So I peeked over the edge of that hole, which was about at my eye level, which means it was close to six feet high. All I could see in there were huge purple mosquito larvae. Now, I'd never seen purple mosquito larvae in my life. Also, they had long hairs all over them. This just about blew my mind, because I knew I had something that was different, that had never been found in California before. By this time I had considerable background in looking at all the mosquitoes that occurred here.

Well, to make a long story short, this again turned out to be a new genus and a new species in California. It wasn't known west of the Mississippi River, and here it was in a treehole in southern California. That was *Orthopodomyia signifera*, which I then wrote my second scientific paper on.<sup>1</sup> People later rediscovered *Orthopodomyia signifera* in California at various spots because they'd never seen it before, and the species wasn't in the usual publications that listed the mosquitoes of California. Now we know it occurs all the way from northern to southern California and also in Arizona. Those are very exciting moments when you're a young entomologist and you see something new, in this case two new genera of mosquitoes not known to occur west of the Mississippi River. So both genus and species were new to the state.

About the only other thing I ever did on general mosquito studies was with Pedro Galindo, who's from Panama. I'll be speaking about him later, because in my opinion he became the outstanding medical entomologist in all of Latin America. He was a graduate student, and we did a lot of very general collecting of mosquitoes in various parts of the state. He did a master's

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<sup>1</sup>William C. Reeves. The genus *Orthopodomyia* Theobald in California. Pan-Pacific Entomologist 1941, 17:69-72.

thesis while he was here, and in that he described a new species of *Culex* mosquito that I'd collected down in Kern County. He named it after me--*Culex reevesi*. It is an honor that you can give to people who discover a new species. Either you do it as an honor, or you do it as an insult. This was an honor. I also had a beetle named after me, a dung beetle, one of the scarabs, and that is *Phyllophaga reevesi*. Larry Saylor named that after me. I'm not sure whether that was a compliment or whether he was telling me what he thought about me. [laughter] I collected two specimens that were mating in Death Valley so he could describe the species in detail. I'm not sure that beetle has ever been collected again. The original specimens are probably in the National Museum in Washington, D.C.

Hughes: Is it a privilege of the discoverer to name the beast?

Reeves: The discoverer, no; the describer, yes. It would be very, very poor protocol for me to have named that mosquito after myself. You give it to somebody who spends their time describing species, and then they describe it. I have a *Culicoides reevesi* gnat named after me by M. Willis Wirth, because I collected that new species for the first time and gave it to him, and it turned out to be new.

Pedro described the species as *Culex reevesi* in his thesis, but a thesis doesn't constitute an official publication. So later M. Willis Wirth and Dr. Richard Bohart of Davis, in the revision of The Mosquitoes of California, both described the species, naming it again after me, because that's what Galindo had wanted. Unfortunately, it turned out that they described the wrong mosquito, and it wasn't the one that Pedro had described.

We learned that the hard way, because Dr. Bernard ["Barney"] Brookman, who was working with me down in Bakersfield, got married, and he and his bride went on a honeymoon trip in lower California. Going by some of the stream beds that come down to the Pacific Ocean, he saw some places that looked very good for mosquitoes. So he came back all excited. On his honeymoon, he couldn't take time to stop to look for mosquitoes, so we had to go back down and see what kind of mosquitoes were there, and we did. We found this mosquito that we couldn't identify from the keys that Wirth or Bohart had put out. We spent quite a bit of time studying that. Finally I said, "Barney, you go get Pedro Galindo's thesis out of the library over in Ag Hall," which is now Wellman Hall, "because I remember a mosquito that looks like this. It may be one that he described as *Culex reevesi*," and it was. So



Brookman and I published a paper<sup>1</sup> and named this mosquito after Bohart so he'd always remember the mistake he made. He had a dim view of this action, as he didn't think it was very proper or funny.

You're always discovering things, no matter how long you've been in the business. We and others are always finding new mosquito species in the state that we didn't know were here before. Or, as we're doing right now, we're dividing up a species into two species. Well, I'm not involved in it directly, but Dr. Bruce F. Eldridge at Davis is describing some new species that we found in our snow mosquito project. So new species are still being found and described, and some of them are even involved with viruses. You never lose your interest in new species, but I also never have gone further into the area of mosquito taxonomy. I don't find it exciting.

Hughes: Were you able to distinguish most of these species by just visual inspection?

Reeves: Yes. You find different characters on them--different distributions of hairs and scales. Different colored scales are located on different sites on the body, on the wings, or on the thorax--wherever.

Hughes: You need a microscope for that.

Reeves: You use a dissecting microscope, not a big, powerful compound scope, but one that will magnify anywhere from twenty-five- to a hundredfold.

Hughes: So a dissecting microscope would be an essential part of field equipment?

Reeves: Well, you can use a hand lens, but that's very limited in what it will do. If you really want to do it right, you have to sit down with a microscope and concentrate. You can tell the different stages of mosquitoes apart. You can separate the larval stages and their species. You can tell the adults of different species apart. You can tell the males and females apart.

Hughes: Were there larval specimens in the collection in Ag Hall?

Reeves: No, they didn't have any larval collections, which was one of the things that was wrong with that collection. You like to have

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<sup>1</sup>B. Brookman, W. C. Reeves. A new name for a California mosquito. Pan-Pacific Entomologist 1950, 26:159-160.

larval samples matched with adult samples so that you can tell the species apart in any stage of their development.

Hughes: How, then, did you recognize the mosquito? Or did you see the adults as well?

Reeves: I saw the adults as well, as I hatched adults of *Orthopodomyia signifera* from the collection of larvae and pupae. These mosquitoes are not necessarily purple in the aquatic stage, and that was another interesting sidelight in the research. I'd never seen a purple mosquito before, and I couldn't understand why they were purple. The descriptions of the species from the eastern United States said they were a whitish-yellow color. Well, to make a long story short, in that particular treehole a purple bacteria named *Thiocapsa* was growing. The purple pigment in those bacteria had been eaten by the mosquito larvae and incorporated into their skin as a by-product of their nutrition. Soon after you took them away from the purple bacteria, they became yellow. So you could make them turn purple or yellow. It just happened that the bacteria were in the treehole and made the larvae purple. That increased the excitement.

Hughes: You don't have a microbiological background. How did you know what the bacteria were?

Reeves: Because in the water that I collected with the mosquitoes, you could see that the decaying leaves and wood were covered by this purple material. So I made slides. Well, as a matter of fact, at that time I was taking courses in microbiology, and I grew the bacteria in an artificial culture medium. As a matter of fact, just to be very scientific about it, I sort of phoned up an experiment. I cultured the bacteria, fed them to the mosquito larvae, and they turned purple. That tied the whole thing up.

Hughes: Was that a new finding, that pigment from bacteria was incorporated by mosquito larvae?

Reeves: I don't know; I never worried about how new it was. At that time it was exciting, and they were pretty. I colonized the mosquito by feeding it on me and got some second generation progeny, but I didn't know what to do with them, so I got rid of the colony. As a matter of fact, that colony had an interesting distribution, because I just couldn't bring myself to throw them away. I was making a trip down to Bakersfield about that time, and there was a bamboo grove at the Bakersfield Country Club. So I put the mosquitoes there.

Hughes: Oh, you didn't!

Reeves: Yes, there were huge bamboo stumps in the grove, and they had a sprinkler system that kept filling them with water. So I thought that was a nice place to put these mosquitoes. In today's world we wouldn't dare do that. I liberated them in 1943. In 1945 Dr. Brookman joined our staff and became very excited when he was in the field one day. He said, "I just made a new discovery. I found *Orthopodomyia* in Kern County." I said, "Where? Up at Bakersfield Country Club?" He said, "Yes, how did you know?" I said, "Well, Pedro Galindo and I put them out there in 1943." And they are still there.

Hughes: Isn't that amazing.





#### IV EARLY CAREER

##### Consultant Positions for the U.S. Military

Hughes: Shall we skip to the consultant positions that you had in World War II?

Reeves: Yes. We found ourselves in an interesting situation, because in 1943 I went full time on the staff of the Hooper Foundation. In '41-'42 when we were doing the Yakima, Washington, field project I was just a summertime employee. I was still finishing up my Ph.D. degree in Berkeley and moving back and forth doing teaching assistantship work in Berkeley and the research on my thesis over in Hooper.

But in '43, when I finished the thesis, the war was on and everybody was going into the armed forces. If you had a degree you could be commissioned as an officer. It seemed a little preferable to be a commissioned officer rather than a private, both moneywise and otherwise. So I was prepared to follow all of my peers who were going into the army or were already in the army, which they almost all were. My former roommate, Deane Furman, was in. Tommy Aitken was in. Everybody was.

In 1943, the army became extremely interested in mosquito-borne viruses. There were outbreaks of mosquito-borne diseases in our troops and a big dengue outbreak in Hawaii. The army found out that the only people in the United States that were really working on these viruses and making some progress in understanding them were Dr. Hammon and myself at the Hooper Foundation.

It was about this time that the army formed the Armed Forces Epidemiological Board and various commissions, like the Commission on Viral and Rickettsial Diseases, to provide civilian advisors to the armed forces as problems occurred, or to have civilians

available to do special research projects that couldn't or wouldn't be done by personnel in the armed forces. Most of the manpower of the armed forces was overseas, not in research labs in the United States.

So about this time we got word that we were frozen in our civilian positions by the Surgeon General of the Army, General Steve Simmons. He wanted to know where we were and when we were there, in case he needed us. If we got into the commissioned corps and got distributed in that system, he could probably still find us, but we'd be doing something else and wouldn't really be available to do special research projects.

So I wound up as a twenty-seven-year old kid frozen in a civilian position in 1943 and through the rest of the war. My draft board was anxious to draft me and was constantly giving me welcome letters which all said it was my time to go. The welcome letters would come, and I refused to take any position on this; if I had to go, I was going to go. But I was going to resent going as a private; I didn't think it necessary that I do that. It was very difficult to be a twenty-year-old, able-bodied person in this civilian community where other persons your age were out in the armed forces. So we were frozen, and any time it got too hot with the draft board, the War Manpower Board would intervene and say, "Don't worry about it; he's doing what we want him to do, and he's doing a valuable service."

Now, what happened as a result of this was that when there would be an epidemic of a mosquito-borne virus anywhere in the continental United States, and a federal agency would get a request to do something about investigating it, the army would say to us, "Go investigate this."

### Western Equine Encephalitis

Reeves: So in 1943 we were working in Kern County on research, and word suddenly came through from Washington that there was an epidemic of western equine encephalitis in horses in Nebraska, of all places. "Go investigate it." So within forty-eight hours we had pulled anchor at Bakersfield and gone up to San Francisco and gotten our gear all together on a trailer. Pedro Galindo was the only person I had working with me in Bakersfield that year, because during the war it was very difficult to get anyone else. He was a member of the Panamanian Consulate Service in San Francisco, and this protected him from being drafted by the U.S.



Army. So we just took off, and I was very fortunate to have Pedro.

So we'd go off whenever there was a horse epidemic; and we'd usually get there right at the tail end of it. We did isolate the western equine virus from *Culex tarsalis* and gave the local people advice on what should be done to prevent this disease.<sup>1</sup> Unfortunately, by that time the first frost had occurred, so the epidemic was over, and we told them it was over. That was an interesting experience.

Then in 1944 there was an epidemic of western equine encephalitis in Oklahoma. We immediately got orders to report to Fort Reno, which was the last remount station for the U.S. Army. We get there and find out that one of the reasons they're really concerned about this epidemic is that General [George S.] Patton has liberated the Lippizaner horses. When he went charging through Austria or wherever they were, he found them, liberated them, and shipped them all back to the United States, and here they were at Fort Reno. They were a national resource for Austria. Frankly, our government was very embarrassed that he'd shipped them back to the States, maybe because he wanted to keep them for himself.

Anyway, they didn't want those horses to get this disease, and they wanted an investigation, so off we went.<sup>2</sup> We spent a month in Ada, Oklahoma, which is the end of everywhere--a great experience--and collected a lot of mosquitoes. But again we got there when the first frost had happened. By that time we had recommended that they vaccinate all the horses, so they weren't going to get the disease anyway.

Hughes: Were they using the vaccine at that time?

Reeves: Yes. The vaccine for western equine encephalitis actually was developed in the late 1930s and by 1944 was quite effective.

Hughes: It was developed at the Rockefeller?

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<sup>1</sup>W. McD. Hammon, W. C. Reeves, P. Galindo. Epizootology of western equine type encephalomyelitis: Eastern Nebraska field survey of 1943 with isolation of the virus from mosquitoes. *Am. J. Vet. Res.* 1945, 6(20): 145-148.

<sup>2</sup>W. C. Reeves, W. N. Mack, W. McD. Hammon. Epidemiological studies on western equine encephalomyelitis and St. Louis encephalitis in Oklahoma, 1944. *J. Infect. Dis.* 1947, 81:191-196.

Reeves: No, it had nothing to do with the Rockefeller Foundation. It was developed originally at Duke Medical School. The yellow fever vaccine was developed at the Rockefeller Foundation. That's the only vaccine they've ever developed that I know of.

Actually, the Lederle Laboratories in New York were the ones that finally really put the western and eastern equine encephalitis vaccines on the market wholesale. Dr. Herald Cox was the director of their research program. He'd worked on western encephalitis earlier at the Rocky Mountain laboratory in Hamilton, Montana, before he went with the commercial company. It's a very good vaccine; it's a vaccine originally developed from infected embryonated chicken eggs. They then formalinized it to kill the virus, but they still had enough antigen in it to produce antibodies.

#### Japanese B Encephalitis in Okinawa. 1945

Reeves: In 1945, we again were down in Kern County, right in the middle of the first big DDT experiments to control encephalitis. Suddenly I got a phone call from San Francisco, and in a very circuitous way I was told to get back to San Francisco as fast as I could. They couldn't tell me why. I got back, and we had these classified orders, top-secret orders, for us to report to Okinawa in the shortest possible time, because there was an outbreak of Japanese B encephalitis.

Hughes: You and Hammon?

Reeves: Yes. So we have orders, but we can't tell our wives where we're going because they don't want the Japanese to know what we're doing.

The first thing we have to do is get all of our immunizations brought up to date so we can meet the armed forces requirements for us to go overseas. So in one day we got typhoid, tetanus, cholera, and I don't remember what else. We got four immunizations, two in each arm, so we could get our orders and have everything filled out so we could go and not be a risk to ourselves or the army.

Meanwhile, we had to buy civilian-type uniforms that just said "U.S." on them but didn't have any other identification. We got our identification papers. I was given the assimilated rank of a full colonel. I was twenty-nine years old. Pretty heady stuff to be a colonel. That was in case you were captured. It

established the rights you had if you were captive. It turned out that the worst thing to do was to have a lot of rank if you were captured by the Japanese, because they gave you a lot of attention, and not necessarily the attention you wanted.

We hadn't quite gotten out of town when an army courier came through again carrying stuff that had to be re-iced before it went to Okinawa. So I opened up the package, and it was Albert Sabin's first very crude mouse-brain Japanese B encephalitis vaccine. He was out in Okinawa. We didn't know it at the time, because he hadn't touched base with us when he went through town. He'd made this very crude vaccine for the Japanese B encephalitis epidemic, so he wanted to go out and evaluate his vaccine.

I went to Hammon and said, "Hey, we've got Albert's Japanese encephalitis vaccine. Do you really think we ought to take it? We're going out to study this disease." We'd been doing research on Japanese B as another part of our assignments from the army, and we had the virus in our lab. We decided we ought to take the vaccine, so we broke out an ampoule of it and diluted it.

"Well," I said, "what are we going to do?" He said, "Our arms are sore from all these other vaccines, so we'll have to take it someplace else. We've got plenty of subcutaneous tissue in our tummies. We'll take it there. I said, "OK." So he loaded up a syringe and jabbed me, and it was just like a red-hot poker in there. It was full of formalin. When you take it there, your sympathetic nervous system reacts, and you want to go to the sink and relieve yourself of everything that's in the way. I squealed like a stuck pig. He said, "Oh, come on. You're always bluffing. You're always pretending something hurts when it doesn't. Give me my shot." So this entomologist loaded up a syringe and gritted his teeth. "Wooooo!" he said. He headed for the sink. "Why didn't you tell me it hurt?" So we got our vaccine.

They were very concerned about biological warfare in the 1940s, and indeed there was some reason for thinking that the Japanese were sailing hot-air balloons in the jet stream across the Pacific. They dropped fire bombs this way. It was always classified, and I'm not sure whether or not there was any evidence of a bacterial agent being introduced that way. I guess that plague was a possibility, but it already was here, so it wouldn't have been an effective weapon.



Hughes: Dr. Elberg talks about that in his oral history.<sup>1</sup>

Reeves: We were not privy to that classified information, but indirectly we certainly were very conscious of it.

Hughes: Did it indeed turn out that the Japanese were working on germ warfare?

Reeves: I don't really know. I'm not sure anything like that was ever declassified.

I was evaluating the question if Japanese B encephalitis virus was introduced here, would our mosquitoes be able to pick it up, and would the birds that we have here be suitable hosts? Indeed, I showed that at least seven of our common species of mosquitoes in California were very efficient vectors of the Japanese B encephalitis virus in the laboratory. Our common dicky birds, like house finches, house sparrows, and some of the blackbirds, also were very efficient hosts for these viruses. They had a lot of virus in their blood when infected. So the evidence was quite clear that if the virus was introduced here, we could have a real problem. That was part of what we were doing for the armed forces and were funded to do.

We went to Okinawa in 1945 and arrived there right about on V-J Day. It was sort of a hairy place to be at that stage as a civilian. I'd made the mistake of jumping off the back of a truck with all my gear on in Guam and pulled the ligaments in my knee, so I had a knee that was twice its normal size by the time I got to Okinawa. I got hospitalized with a cast on it, not of my volition, but an army colonel said that if they had to hog-tie me, that's what they were going to do. So I spent most of my time in Okinawa as a real consultant. I was flat on my back in bed when they brought in mosquitoes. Hammon was out collecting mosquitoes. He'd bring them in, and I'd get on my crutches to go down to the lab and sit down at the microscope and identify them.

Hughes: Did you have any trouble identifying them, considering they must have been mosquitoes that you'd never seen before?

Reeves: I pretty well knew the mosquitoes in Okinawa, plus the fact that the place had already been invaded; very competent entomologists there by then. We knew pretty well in advance what mosquitoes would be there, and we knew which species were possible vectors.

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<sup>1</sup>Sanford S. Elberg. Graduate Education and Microbiology at the University of California, Berkeley, 1930-1989, Regional Oral History Office, University of California at Berkeley, 1990.

Hughes: Was Japanese B a problem with our troops?

Reeves: There was an epidemic in 1945. That's why we went out.

Hughes: In our troops?

Reeves: In our troops as well as in the natives. This was why they took the vaccine out. The vaccine didn't get a good field trial there. We had maybe thirty cases in our troops, and there were possibly hundreds of cases in native children. The first hospital we worked in was a military hospital for the Okinawans, and they had wards filled with Japanese encephalitis cases. The U.S. Navy research unit made a movie of it, but Albert Sabin from the army was the central character, because he moved in and took over the center of the stage.

By the time we got there, we really couldn't do most of the research that we wanted to do. I was laid up, so I couldn't go out and make big mosquito collections, and we didn't have any dry ice, so we couldn't get specimens back that were in good shape for virus isolation. Pigs were strongly suspected of being one of the primary hosts of this virus, and we couldn't find a pig on the whole island because the military had killed and eaten them. But it was an interesting experience.

That's where I first met Wilbur Downs. Wilbur Downs was in charge of preventive medicine for the armed forces for that theater at that time. He'd worked his way all the way up from Guadalcanal without ever getting back to the States. He'd been on [General Douglas] MacArthur's staff, starting at the lowest rank. I guess he was a lieutenant colonel at that time and getting ready for the invasion of Japan. I had corresponded with him before on the malaria work that he was doing in Trinidad, but I'd never met him, and that was the beginning of a very close relationship we've had ever since.<sup>1</sup> He was not too pleased to see us, as a couple of civilians being dumped in there wasn't any help to him. He didn't need us for any reason he could think of. Probably right.

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<sup>1</sup>Dr. Downs died February 16, 1991. Obituary Tropical Medicine and Hygiene News. 1991. 40:No. 2: 66-67.

The Commission on Viral and Rickettsial Diseases, Armed Forces  
Epidemiological Board

Reeves: At this time there was a Commission on Viral and Rickettsial Diseases as part of the Armed Forces Epidemiological Board, of which Hammon was a member. Dr. John Paul at Yale was the head of the commission. Their concern was very wide, covering what civilians could do to assist the armed forces in virus problems. So they covered the arboviruses, such as yellow fever, dengue, Japanese B, western and St. Louis encephalitis; and poliomyelitis, because we didn't have a polio vaccine at that time. Hepatitis viruses were extremely important. Saul Krugman in New York was one of the primary people on that commission, because he had a study of these viruses in New York at Willowbrook orphanage. Also Paul Havens from Pennsylvania; Robbie Ward from USC [University of Southern California]; he was head of pediatrics there. These were all people working on hepatitis. John Enders from Harvard was on the commission because of his broad competence. He and his associates Tom Weller and Fred Robbins received the Nobel Prize for their work on development of tissue culture techniques applicable to virus research.

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Reeves: The commissions at that time were allowed to have only one full member from any given organization. So I was an associate member from 1945-1959, but I went to all the meetings. When John Paul was no longer the chairman, Bill Hammon was made the chairman of the virus commission. I was made a full member in 1959 and was a member as long as the commission was active, which was up until 1973, when the commission was terminated.

Reeves: I continue to be a consultant to the armed forces R&D command [U.S. Army Medical Research and Development Advisory Committee]. As a matter of fact, I've just had a request for an updated CV [curriculum vitae], because they're renewing my appointment as a consultant on the army research and development panel on virological diseases.<sup>1</sup>

Hughes: That would probably come later in our discussions, wouldn't it?

Reeves: Only in the sense of the reviews I've done for them. When they dissolved the commissions, they established review groups in various areas of concern to the armed forces to review their

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<sup>1</sup>The full title was, Ad hoc Review Group on Viral Diseases, U.S. Army Medical Research and Development Advisory Panel.



contracts with civilian groups. From 1973 to 1977 I was a member and from 1981-1983 the chairman of the Ad Hoc Review Group on viral diseases. I'm not a virologist, but anyway I was made the chairman. Our task was to assure that they got good, tight contracts, whether they were on influenza, polio, or mosquito-borne viruses. They were covering a wide array of diseases. I did that for two years, and then they decided they wanted me to be responsible for the Ad Hoc Review Group for Medical Entomology.<sup>1</sup> I was chairman until it was dissolved, as we'd done our job.

#### U.S. Army Medical Research and Development Advisory Panel

Reeves: Subsequent to that time, I've been asked by General Russell to review specific programs and to advise his command of what should be done. A year and a half ago, in 1989, we finished a review of all the medical entomology programs within the armed forces all over the world--army and navy, the whole works--and submitted a report to General Russell on our findings.<sup>2</sup> I had four other people who worked with me as chairman of that committee, an unusually competent committee.

Hughes: How were the committee members chosen?

Reeves: That's an interesting story, and it made this one committee very interesting. When General Russell told me he wanted me to be the chairman of this committee, I said, "Who's going to be on the committee?" He said, "We haven't made a decision yet." I said, "Could I make suggestions, then?" "No," he says, "you can't make suggestions. If you did, you'd have nothing but a bunch of arbovirologists and medical entomologists. You can have the medical entomologists. You can't have a bunch of arbovirologists; you're too prejudiced." I said, "Why do you say that? Would you like to know who I would recommend?" He said, "Yes, who would you recommend?" I said, "I'd like to have Lou Miller on the committee."

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<sup>1</sup>Ad hoc Study Group on Medical Entomology, 1981-1983, U.S. Army Medical Research and Development Advisory Panel.

<sup>2</sup>W.C. Reeves, J.D. Edman, B.F. Eldridge, L. Miller, J.H. Oliver. Review of the medical entomology research program within the United States Army Medical Research and Development Command. Submitted March 10, 1989, pp. 1-41.

Now, Lou Miller happened to be the top person in malariology in the National Institutes of Health, a very competent person. General Russell said, "Do you think you can get him?" I said, "I can ask him. He also happens to be the president-elect for the American Society of Tropical Medicine. I guess that doesn't make any difference. You think he's all right?" "Yes, he'd be all right."

General Russell said, "Who else would you like to have?" I said, "I'd like to have Jim Oliver." "Who in the hell is Jim Oliver?" I said, "Well, he happens to be the top person in this country on ticks and on tick-borne diseases. It seems to me that the armed forces ought to be interested in ticks and tick-borne diseases. It just happens he's also the president-elect for the American Entomological Society. Would he be all right on the committee?" "Well, if you can get him, he'd be all right." I said, "I think I can get him."

And we went on down my list. I wanted Bruce Eldridge because he is a very competent medical entomologist who was in charge of this program for the army for some years. Now he's a civilian on the Davis campus of the University of California. And I wanted John Edman, chair of the entomology department of the University of Massachusetts, who was particularly interested in leishmaniasis and other diseases that are insect-borne. Anyway, Russell finally said, "All right. If that's who you want for your committee, if you can get them to say yes, you can have them." [laughter]

Now, that shows you one way a committee gets elected, okay? I've had other committees that I've inherited, and I wasn't given any choice in who was on them. Generally speaking, I think most organizations are perfectly willing to have chairpersons suggest who might be the best person or persons to be added when they have vacancies on their committees. Sometimes you'll take over a committee, and within the next year or two you'll have a chance to change the membership. People generally are allowed to be on one of these committees for only three years, and then they have to go off. So whether it's the National Institutes of Health or the armed forces or any other organization, if you're asked to head up one of their committees, you get a lot of voice in its composition. After all, you want people who can work with you. That doesn't mean you always will agree.

I've been on endless committees, advisory capacities, for the armed forces, and still am. As an advisor, you're on a completely different basis from the people who are in the armed forces, because you're a civilian, and there's a difference between being a civilian and being in the armed forces. You're an outside person coming in to review things. There's a lot of concern by

staffs of these organizations about the competence of outside people.

You get none of the "perks" for your services as you would if you were in the armed forces. I've never been able to get a loan to help buy my house or anything like that. But I don't resent it at all. As a civilian, I frequently didn't get paid at all or as much as the people in uniform. The salary I was receiving from the university during World War II was much lower than if I'd been a commissioned officer. When I started, my salary was well under \$300 a month. You can't tell me that a lieutenant colonel in the army is getting less than that.

Hughes: Did you have children at that point?

Reeves: Yes. The first son, William C. Reeves, was born in 1943 in San Francisco. He's forty-seven now. My second son, Robert Flay Reeves, is forty-four and also was born in San Francisco. The third son, Terrance Moulton Reeves, is forty-one and was born in Berkeley.

Hughes: Your wife was no longer teaching, was she?

Reeves: No, she was taking care of the kids, and particularly me.

#### Hammon's Consultant Work for the Army

Hughes: When Hammon moved to Pittsburgh in 1949, he still continued to serve on these committees?

Reeves: His particular research interests at that time were in developing vaccines for Japanese B encephalitis, because he was no longer involved in field studies of the type we were doing here. He newly discovered the hemorrhagic fever and shock syndrome aspects of dengue fever when he was out in the Philippines for the army investigating a polio epidemic in the armed forces. He was called into the hospital to look at these peculiar cases of a very severe disease that was killing Filipino children. That immediately developed a new area of concern for the Commission on Viral and Rickettsial Diseases, as it was a form of dengue, a new disease in the sense of its severity. Before that, the textbooks said nobody ever died of dengue; they just wished they had because it hurt so much.

Then he did research in Thailand for the virus commission on a number of projects on dengue and continued attempts to develop a Japanese B vaccine by attenuating the virus. That vaccine project



didn't succeed, so he said, "There's no point in my continuing this work. I'm not successful at it." Actually, by the time he retired some years later [1973], he was pretty much out of the work with the Armed Forces Epidemiological Board, and I continued. I think I continued because there are a lot of physicians concerned with these problems but not a large number of entomologists who are that concerned. They thought they had an entomologist when I was appointed. However, I think I'd forgotten most of the entomology I ever knew. I'm not a good virologist. I'm not a good entomologist. Maybe a fair epidemiologist. [laughs] Even that's debatable, too.

Hughes: I don't think very.

### Teaching Medical Students during World War II

Hughes: Getting back to the war, were you not also asked to teach epidemiology to medical students?

Reeves: Yes. Actually, tropical medicine, not epidemiology. What happened was that the primary mission of the medical school in San Francisco, as with most medical schools during the 1940s, was to train physicians who were going to be in the armed forces. So in a medical school class of, say, a hundred students at San Francisco, ninety would be in the armed forces.

The armed forces did a review of the program in San Francisco about 1943. What they found was that the faculty competent in tropical medicine who had been at the medical school were in the armed forces. Herbert Johnson and those sorts of people were no longer there. So the armed forces said, "Look, we're not in the business of sending people to school just to learn to be obstetricians, pediatricians, and this sort of thing. There's a war going on, and we need people trained in tropical medicine. If the medical school does not develop an adequate training program in tropical medicine, we're going to have to move our students. We're sorry if this is going to close your school, but it's probably what we'll do."

So the dean, Francis Smyth, who was a very competent pediatrician, was pretty frantic about this, and his faculty was pretty worried, because they didn't want to close their medical school. So they looked around and found a couple of characters sitting over at the Hooper Foundation. Dr. Hammon was one, and I was the other. They found out that Hammon had had four years of living in the Belgian Congo as a medical missionary. He'd had a

lot of experience with tropical diseases; he'd had them all. He was still carrying amoebic dysentery in his gut. It wasn't bothering him any, so he kept it there as a curiosity. It came in very handy in a teaching program, because you had fresh material to show to medical students or anybody else who was interested. He went down to Panama for a refresher course on tropical medicine in about 1944 to see fresh malaria cases and so on. He had a bout of diarrhea while there which disturbed his amoebic dysentery, so he had to have it cured [laughs], and he couldn't demonstrate the disease to the students anymore.

Hammon certainly knew the virology field. He still knew the malaria field, as he'd trained with Mark Boyd, who was the leading person in malariology in Florida. So the medical school asked him to come over and organize a tropical medicine course. They also drafted me, because I could cover the medical entomology aspects. Arthropod-transmitted diseases, like malaria, scrub typhus, encephalitis, and dengue fever, were extremely important diseases during the war. As a matter of fact, our ability to do something to prevent those diseases probably did almost as much for us in winning the war as other things, because casualties can be extremely high from these infections.

Hughes: Were there vaccines?

Reeves: There were not vaccines for most of these diseases. There was no vaccine for malaria, dengue fever, scrub typhus, or encephalitis. Well, there is a vaccine now for Japanese B encephalitis. We didn't have antibiotics at that time, so we couldn't treat scrub typhus cases and other rickettsial diseases, which we could do now. These diseases had to be controlled by protecting troops from mosquito or other vector bites. It put a very heavy load on the medical entomology field.

Anyway, between the two of us we could handle the teaching of tropical medicine, and we felt very good about being given this opportunity. We had trouble, because the students resented having to learn tropical medicine. They were typical medical students, and they wanted to be red-hot physicians in whatever field, but they didn't want somebody to tell them what their field was going to be. The medical school had pressure from the armed forces that there had to be a significant amount of training done in tropical medicine. They solved the problem in the usual way: they didn't change any of the other teaching program; they just gave us all day Saturday to teach tropical medicine. There'd never been classes on the San Francisco campus on Saturday, ever. Now, we didn't make that decision, but the students assumed it was our fault. Suddenly, they had to go to school all day Saturday.



So we started these courses, and we were working our tails off to make sure these were good courses, including taking them down to see the first marine division which had come back with almost all casualties from diseases contracted in Guadalcanal. This was an unusual experience for a young medical student. The marines told them the facts of life in no uncertain terms.

The students weren't showing up for class and were ducking out. So one day I went over to the sergeant who ran the ROTC [Reserve Officer Training Corps] program, and I said, "Sergeant, we've got trouble. These students aren't coming to class, and nobody knows what to do about it." He said, "I do." So he put out orders that everyone in that class who was in uniform would report in full uniform at 0700 on Saturday and would be marched to breakfast and to class. If anybody left that lecture room, it would not be without a written pass from him to return on time from wherever he went. Anybody who didn't obey these orders was going to be handed a rifle, declared a private in the U.S. Army, and shipped off to the South Pacific. We had wonderful attendance after that. [laughs] After about two weeks of this, and getting to know the students, and getting their reaction to the experience that they were having, I said, "Sergeant, you can stop now. Everything's okay."

For a number of years after that, as I would go out into the Pacific or other areas around the country on projects or on reviews, I'd run into these people, and they would thank me. They would say, "Thank God, we had that course." It was a part of my relationship and a loyalty which I still feel to the armed forces, which are a very important part of our society whether we like it or not. I've had to tell some people who objected to my doing some of these things: "I'll be very candid with you. I'm more concerned with the health of the people who are out there in these difficult situations than I am with yours. If you don't like that, it's just too damn bad." And I really feel very strongly. I've made no secret of it.

Hughes: Did you have enough knowledge to teach the course, or were you boning up as you went along?

Reeves: You have to remember that on the Berkeley campus at that time we probably had the strongest curriculum in medical entomology and parasitology of any university in this country. However, I still had to bone up every day on developments in tropical medicine.

Hughes: Was the curriculum at Berkeley thanks to K. F. Meyer?

Reeves: No. Medical entomology was thanks to Professor Herms, and the medical parasitology was due to people like C. A. Kofoed and



Harold Kirby in the zoology department. We had extremely strong programs in those two fields. Now, I'll grant that Harvard and Hopkins and Tulane also had good programs, but we were certainly one of the big four in this field in this country. Berkeley probably was turning out more better prepared medical entomologists and parasitologists than any other campus in the country.

When I was drafted into this teaching program, I'd just finished my thesis in '43. My Ph.D. was in medical entomology and parasitology. I'd taken all the medical entomology, parasitology, and microbiology courses that were available on this campus. So academically I had the background, plus the fact that we had already been deeply involved in the mosquito and virus work, plus I had close association with other students who were working on ticks, mosquitoes, gnats, or whatever it might be that were transmitting a disease. We had very intensive informal sessions in which students got together to discuss their work. The scientific fraternities I told you about seem to be practically nonexistent today.

We were a very close-knit group, and we didn't just study the diseases that were in California. I remember that the first big term paper I wrote was on African trypanosomiasis, also called sleeping sickness, caused by protozoa and transmitted by tsetse flies. I astounded students for years after that with my depth of knowledge in that area.

We were taught on a very broad front what was known about bacterial, viral, protozoal, and helminthic diseases. I'd been teaching assistant to Dr. M. A. Stewart, who taught the first course in helminthology on this campus. He came down from Davis to teach the class, and he picked me out to be his teaching assistant in '43. My helminthology was fairly limited at that time because it was just what I had gotten in an introductory parasitology course, but I knew a lot more by the end of the semester from being the T.A. in that course. I also was the teaching assistant in the medical entomology course for several years. Then we had a joint course in medical entomology and parasitology, which was sort of an introductory course. It was taught half by the zoology department and half by the entomology department. I was the teaching assistant in the entomology part of that course. So this was the sort of teaching experience and knowledge that was brought into the tropical medicine course in San Francisco. I'm not bragging; I'm just saying that's what we had been required to learn at Berkeley.

### Changing Demand for Entomologists

Hughes: Do you look upon war as an impetus to certain studies in tropical medicine and maybe to virology and microbiology in general?

Reeves: There's no question that a field like medical entomology is an extremely necessary field and one in which there are extreme demands for adequate personnel any time there's a war. That has gone on historically as long as we've had wars, whether it was World War I, World War II, Korea, Vietnam, and currently the Arabian Gulf. They never have enough medical entomologists to go out and do the job. They usually have to take somebody from the general field of entomology and quickly give them a veneer of medical entomology because they haven't had a good curriculum in medical entomology. I think you have a copy of the editorial that I wrote recently about the shortage of medical entomologists. It was the lead editorial in the Journal of Tropical Medicine a year and a half ago.<sup>1</sup>

The shortage is also very widely recognized at this stage in the fields of tropical medicine and entomology. Major organizations, like the American Society of Tropical Medicine and the Entomological Society of America, are making big pushes for training additional people in these fields, not only because of the needs of the armed forces but because of our society's international obligations for health improvement in the Third World. We just don't have the people to be out there. And we can't just depend upon the old people that have been around long enough to have had that experience.

Hughes: Why aren't young people entering the field?

Reeves: There are no or few jobs. If a young person in medical entomology comes to me today and says, "Where can I get a job for sure tomorrow?" I almost have to point them to the armed forces, and some of these people don't want to go in the army or navy. But the Public Health Service doesn't have that many jobs for them. The World Health Organization [WHO] and the Pan American Health Organization [PAHO] can't hire Americans in number. They have a ratio of how many people they can have from different countries. It's a political matter. So they just can't take endless Americans into those international organizations, regardless of their ability, or they'd be outweighing the representation from other countries. If this is done, then those other countries will

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<sup>1</sup>W. C. Reeves. Concerns about the future of medical entomology in tropical medicine research. *Am. J. Trop. Med. Hyg.* 1989, 40:569-570.



withdraw their support from WHO or PAHO. It's a very unfortunate thing that the young person who wants to go into this field now has to really concern himself or herself with the question, "Where's my job going to be?"

When I retired, a lot of people worried about what the School of Public Health was going to do about replacing me for research and teaching of the epidemiology of vector-borne diseases. I had to tell them I didn't think they would do anything, because there are other diseases more important to our society today than the diseases I work on. So they probably would pick somebody who is interested in AIDS or other viral diseases that represent much more of a problem worldwide than what I'm working on. That doesn't mean that what I work on isn't important. When Dr. [James L.] Hardy retires, I doubt very much that he'll be replaced by a person in arbovirology. When that happens, then this school will not have anyone left in this special area, but I trust they will still have strong programs in epidemiology and microbiology.

Hughes: What about the new fields coming along?

Reeves: Like what?

Hughes: Like AIDS.

Reeves: I don't consider AIDS a field. It's a specific disease entity. There are plenty of jobs and plenty of money for research there until that problem is solved. But let's say you're a polio expert today. Where in the hell are you or a student of that disease going to get a job to work on polio viruses?

Look what happened to pneumococcal pneumonia. We got our first full-time dean in the school, Dr. Edward S. Rogers, because he had been a world expert in developing antisera to treat people with pneumonia. And then antibiotics came along. Dr. Rogers was no longer of importance as a researcher, because he was interested in pneumococcal vaccines and pneumococcal membranes and so on and so forth. He was put out of business by antibiotics. Now his field is coming back because they're developing pneumococcal vaccines. However, he went into public health administration, retired, and is now gone. It's a very difficult situation.

The other thing is that the availability of a good curriculum in which to teach these subjects is disappearing very rapidly, because the minute you have a vacancy in something like medical entomology, the department sits down and reviews the vacancy. They're likely to say, "We'll put a higher priority on filling the job in some other area." Once you get rid of those curricula, you



don't have the people who can teach them. Then where do students go for training?

Entomology generally has the same problem. When I was here as a student, there were almost a hundred undergraduates in the department of entomology. There are none now. There isn't an undergraduate major in entomology on the Berkeley campus anymore. Students now get trained in biology departments. I'm not saying there's anything wrong with the biology department as it is, but it's largely concerned with molecular biology or other such developments. That really isn't a criticism, but it doesn't teach people how to go out in the field and study bugs. So students come into a graduate program in entomology, and they've had no training as undergraduates in entomology per se. They may be good biologists in some areas but not in the specific field that concerns me.

Hughes: Is there no undergraduate program in entomology in the UC system?

Reeves: Nothing. I don't think they have any at Davis now and certainly none at the other campuses. So where there were almost a hundred or so undergraduates when I became an undergraduate here, now there are none in that field.

Hughes: Why was there such interest at that particular time?

Reeves: There were endless jobs at the undergraduate level.

Hughes: Well, why were there jobs then, and why aren't there now?

Reeves: Because we weren't into an insecticide domain at that stage, so we didn't have what we thought was a cure-all for everything. We had other types of crop management that were necessary and had to be developed. Biological control was considered to be a very important field in those days. Then we went into the DDT and the organophosphorus era, and that eliminated an awful lot of positions.

The difficulty is that you now get people going into graduate work who don't have a couple of solid years of related undergraduate background. Basic courses in insect morphology, taxonomy, and so on, that were taught at the undergraduate level, may not even be available in the department. People have to pick them up some other way, if that's what they want to do. So it's a bad scene in that regard. You can't turn the tap on and off. Retreading people from one field to another field is not the best way to get them trained.

In today's world, people have to go where there are jobs. The fact that you like bugs is just like somebody who goes to a veterinary school "because I love animals." That's the worst reason to go to vet school. Loving animals has nothing to do with veterinary school. Just like saying, "I love people; I want to be a physician." "I love bugs; I want to be a bugologist."

How much parasitology, how much tropical medicine do medical students know? There's no demand for it. It's an elective course. The only school that I know right now that I consider has a strong program in these fields is the one that Wil Downs established at Yale. He shook the money tree and got donations so that he could develop a summer program to send medical students out into international areas, someplace where he had made a contact for a student to do a summer project.

We used to have up to four or five medical students each summer down in Bakersfield. They weren't down there just being medical students. I mean, they were out learning to collect bugs and birds or whatever they wanted to learn about in field virology and epidemiology. And they enjoyed themselves. They also had to spend some time in the hospital running down clinical cases of polio and encephalitis for us, and we certainly had some cases for them to run down.





## V RESEARCH ON ENCEPHALITIS AND RELATED TOPICS

### Arbovirology before World War II

Hughes: Could you summarize what was known about the arthropod-borne viruses in the 1930s?

Reeves: This is covered in a number of publications that I've given you.<sup>1</sup> Basically, what it came down to was that at the time that Dr. Meyer came into this field in 1930, little was known.

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Reeves: When he isolated western equine encephalomyelitis virus from the brain of a horse from Merced in 1930, there were only seven other viruses that were known to be arthropod-borne in the entire world. Those causing disease in people were classical diseases like yellow fever and dengue fever. The other five viruses were only known to cause disease in animals, not in man. Only one of these animal viruses was known to occur in North America, and that was vesicular stomatitis virus, which is still here.

The other arboviruses that were known at that time were Rift Valley fever, African swine fever, Nairobi sheep disease, bluetongue in Africa, and louping ill in Britain. Other than

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<sup>1</sup>See, for example:

William C. Reeves. The discovery decade of arbovirus research in western North America, 1940-1949. Am. J. Trop. Med. Hyg. 1987, 37(3) suppl., 94S-100S.

William C. Reeves. Delights and delusions experienced in 50 years of arbovirus research. R.R. Parker Memorial Address. Typescript [n.d.] 11 pp.

these, no other viruses were known to be transmitted by arthropods.

Meyer added a great deal to knowledge of insect-borne diseases the minute he found western equine encephalomyelitis virus, except it wasn't known to be insect borne. He suspected it might be, and the reason he suspected this was the fact that the horse cases always occurred in the summertime. The minute there was a freeze in the fall, the disease would disappear, as did the insects, particularly mosquitoes.

You have to realize that this was a really important disease at that time. As I've said, there were thousands and thousands of horse cases per year, and we were dependent upon a horsepower economy in agriculture. These epidemics continued annually through the thirties, and they occurred all over the western United States. There also were some cases in the eastern United States. A few years after Meyer made his discovery, Drs. [Malcolm] Merrill and [Carl] TenBroeck discovered eastern equine encephalitis. They differentiated eastern equine encephalitis, in Massachusetts, from the western virus in California. It's interesting that Dr. Merrill, who did that early work, later became the director of the laboratories of the California State Health Department here and the state health officer.

Now, the result of what Meyer did at that particular time was that he suspected that there were arthropods involved as vectors, and he suspected that humans were involved, because he found a disease very similar to the horse disease in some veterinarians who were associated with cases of horse encephalitis. However, it wasn't known how they got it. R. A. Kelser of the U.S. Army had a colony of *Aedes aegypti* mosquitoes, the yellow fever mosquito, so in the mid-thirties he fed them a western virus and showed they could transmit it by bite to horses or to guinea pigs. And this was repeated by some other people. Merrill and TenBroeck on the East Coast showed that eastern virus could be transmitted that way by *Aedes* mosquitoes. So the textbooks began to say that *Aedes* mosquitoes were the principal vector of these viruses. However, people studied epidemics and collected lots of *Aedes* mosquitoes off horses, but they never could isolate the virus from them.

Vaccines were developed in the late 1930s for horses, very crude originally, but they got refined very rapidly. There was pretty good evidence even in the early 1940s that the vaccine probably was going to be relatively effective in horses. At first this was just formalin-treated brain suspensions from rabbits, mice, guinea pigs, and so on. The best vaccine was grown later in embryonated chicken eggs.

The Yakima Valley, Washington, Encephalitis Study, 1941-1942

Hughes: What about the studies in the Yakima Valley in the early 1940s?

Reeves: As I told you, in 1940 there was an epidemic and an epizootic of encephalitis in the Yakima Valley of Washington. It included both western equine and St. Louis encephalitis viruses. A request came through from the Washington State Health Department to Dr. Meyer to send somebody to investigate this. The only person he had at that time on his staff was Miss Beatrice F. Howitt, and she was not a field person at all and would be dependent upon getting people from the health department to collect specimens for her. She didn't know how to collect mosquitoes at all.

At that time Dr. Meyer had decided he'd develop a unit at the Hooper Foundation to study central nervous system diseases and broaden out what Miss Howitt was doing, namely, to get more into field epidemiology. That's when he went to Harvard and met with Hans Zinsser, and Zinsser suggested to him that Hammon would be a good candidate for the Hooper job. So Hammon took this job, and Dr. Meyer intercepted him at the Rocky Mountain Laboratory of the National Institute of Allergy and Infectious Diseases, where Hammon was visiting to find out what they were doing in research on encephalitis viruses. Meyer told Hammon, "You go to the Yakima Valley and send your family down here to San Francisco." So Hammon went to the Yakima Valley and got good clinical material from the cases.

The way Miss Howitt was diagnosing cases, you'd get a single blood sample on a case, and she tested it by the mouse neutralization test. If it was positive for western virus it was a western case; if it was positive for St. Louis virus, it was a St. Louis case. If it was positive for both, it was a double infection. Hammon was convinced that it was necessary to have what we call an acute phase specimen taken right at the onset of the disease and then have a convalescent sample to show that there was a rise in antibody titer, which would be good evidence that there was concurrent infection with that virus.

We also knew there was a St. Louis virus, because in 1933 there had been a large epidemic of over a thousand human cases of a new virus disease in St. Louis. It was in the summertime, and they had isolated a new virus, which was St. Louis encephalitis. [J. P.] Leake, [E. K.] Musson, and [H. D.] Chope were the public health service group that did that. There also was a Rockefeller Foundation group (C. Armstrong and R. D. Lillie) there, and they isolated lymphocytic choriomeningitis virus, another virus that



infects the central nervous system, from some of their cases.<sup>1</sup> The Public Health Service team had a major disagreement about how St. Louis equine virus was transmitted. Leake, Musson, and Chope were determined that this disease was like polio and probably could be transmitted directly from person to person or was transmitted by water with fecal contamination. And their official report said that.<sup>2</sup>

Hughes: Why were they so convinced of that?

Reeves: Well, they thought the epidemiological evidence showed it. They found that infected people had associated with each other. Polio also was seasonal. There was a lot of polluted water in the area that year which could have led to this distribution.

Dr. [L. L.] Lumsden, who was another physician in the Public Health Service group, was equally convinced that mosquitoes were the vectors. He could go to where the cases were occurring and find mosquitoes breeding in foul water. There was a drought that year. That meant that a lot of the water going down the stream beds and drains would pool up, which made an ideal place for *Culex pipiens* mosquitoes to breed. He wrote a report on this which was squelched officially; it wasn't published until much later. But every one of us who were in the business by the late 1930s had a copy of Lumsden's report in our hands. We knew about his interest in mosquitoes. Lumsden's report was published by other workers in this field in 1958.<sup>3</sup>

Hughes: Who suppressed his report?

Reeves: Dr. Leake, who was the head of the research team, believed in the other idea, and so he suppressed it because it didn't agree with his idea. So the official report by Leake, Musson, and Chope didn't even include Lumsden as an author. They squelched him. The interesting thing was, when that project was over, the Public Health Service sent him as a commissioned medical officer to

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<sup>1</sup>C. Armstrong and R. D. Lillie, "Experimental lymphocytic choriomeningitis of monkeys and mice produced by a virus encountered in studies of the 1933 St. Louis encephalitis epidemic," Public Health Reports 1934; 49: 1019-1027.

<sup>2</sup>J. P. Leake, E. K. Musson, H. D. Chope, "Epidemiology of epidemic encephalitis, St. Louis type," Journal of the American Medical Association 1934; 103: 728-731.

<sup>3</sup>L. L. Lumsden, "St. Louis encephalitis in 1933: observations on epidemiological features," Public Health Reports 1958; 73: 340-353.

Yakima, Washington, to organize a local health department there in response to a request from the Washington State Health Department.

Hughes: What was the rationale there?

Reeves: Sort of like sending him to Siberia. Unfortunately, Dr. Lumsden wasn't still in Yakima in 1941 when we proved mosquitoes were the vectors of St. Louis equine encephalitis. I think it would have pleased him.

Hughes: I presume your group wasn't paying attention to the non-mosquito dogma?

Reeves: We were hooked on Dr. Meyer's hypothesis as much as anything.

Anyway, Dr. Hammon knew there were two viruses in Yakima, as there was a diagnostic test for both western and St. Louis encephalitis. Hammon confirmed the diagnosis on such cases and thus differentiated the two diseases.

Hughes: You mean clinically?

Reeves: Not clinically. He still could not differentiate them clinically. As a matter of fact, a good proportion of poliomyelitis cases were very difficult to separate from St. Louis encephalitis. The National Foundation for Infantile Paralysis decided to send an expert from Harvard Medical School to Kern County in 1943 to teach us how this should be done. Fortunately, we had cases there that we'd been able to get the diagnosis done on--polio cases in which we had isolated polio virus from feces, and St. Louis and western cases we'd diagnosed serologically. Dr. Hammon gave this guy the blindfold test, and he flunked. This was the world's leading expert on central nervous system diseases. He thanked us very nicely. He said, "I learned a lot on this trip, and I'm glad they sent me out." Anyway, he couldn't differentiate some of the cases. Some he could, but Dr. Hammon could, too.

Hughes: How was Hammon doing differential diagnoses?

Reeves: By doing serological tests on both acute and convalescent phase sera, and this basically became the mode of doing a good diagnosis. We had to use a mouse neutralization test in adult mice. That meant you had to take the serum and mix it with a known amount of virus and see if the serum inactivated the virus so the mouse didn't get sick. If the patient had antibodies, you protected the mouse from the disease.

Hughes: And that was a test already in existence?

Reeves: It was the only test; there was nothing else. The complement fixation test and the suckling mouse neutralization test had not been invented yet. The hemagglutination inhibition test and all of the fancy new ones that we have now hadn't been invented yet. So your limit was one test. It was not practical to try to isolate the virus from the individual, because it isn't in the blood at that time; it's in the central nervous system. And if they don't die, it's a little hard to get a central nervous system sample to test.

Hughes: Did you know that the virus was not in the blood?

Reeves: Yes.

Hughes: Because you just couldn't find it?

Reeves: Couldn't find it. Now, if a person died, you could isolate it from the brain, but you didn't have that many people dying, or you didn't get the sample, or it was too late into the disease and they died of complications such as pneumonia rather than the acute disease so that the virus no longer was there.

Hammon also collected blood samples from domestic animals--cattle, chickens, and things like that--and some of them had antibodies to western or St. Louis virus. He collected no mosquitoes at all.

That winter he was back in San Francisco, and it was obvious that he wanted to go back to Yakima the next summer to pursue the various avenues of investigation that were left to be done. The obvious ones were to identify cases and find out how extensive they were, try to find out where the infection was coming from, and was the idea that *Aedes* were vectors right or wrong. Or was the *Culex* idea right or wrong. Or was everything we thought we knew right or wrong.

Hughes: Had Lumsden been saying *Culex*?

Reeves: He said *Culex*. Kelser and company, and Merrill and company, said *Aedes*.

At this time, Hammon looked around to try to find an entomologist. He came to the entomology department in Berkeley, and there I was sitting, because I'd been trying to transmit western virus by mosquitoes in the lab and getting my second thesis project set up, I hoped. Plus the fact that I had been very interested in learning all I could about the mosquitoes of California, how to identify them and so on. So he said, "Great. Come on up to Yakima with us and do the field aspect of the



problem." I said, "I can't do it, because I promised Miss Howitt that I'd go to Kern County to do the same thing." As I told you in the previous interview, that's when Dr. Meyer said, "Reeves, you're going to Yakima. You're not going to Kern County." That took care of any dilemma; I didn't have to make the decision.

So we agreed that I'd cover the entomology aspects. I needed additional help; I couldn't do it all myself, because we'd never done this sort of thing before. We didn't even know what we were doing. But one thing we agreed on; we didn't want to just look at *Aedes* mosquitoes, we didn't just want to look at *Culex* mosquitoes. We decided we wanted to look at every blood-sucking insect or arthropod that we could, including ticks and everything else. We wanted to make sure we didn't miss whatever was going on.

#### Recruitment of the Team

Reeves: We tried to get Tommy Aitken to join us, and he wanted to stay in Berkeley, as I told you before. Barney Brookman was another graduate student here, and he was interested in a wide variety of insects, including mosquitoes, so we talked him into joining the staff. Well, it wasn't very hard in those days to talk a student into taking a job for the summer. This was just a summer job, and Barney and I were the entomological group. We knew that we also wanted to really go after the animals, so we added a graduate student in zoology, Johnny Gray, and two veterinary students from Pullman, Washington, to assist him. Johnny was in the zoology department here, so we had a zoological team that would collect a wide array of animal blood so that we could determine which species were involved.

We also had the problem that another person had been asked to come to the Yakima Valley area, and that was C. M. Gjullin from the U.S. Department of Agriculture. The health department was concerned, because if it had an epidemic for a second year, it wanted to be able to develop a mosquito control program. First we had to prove that mosquitoes were involved. So Mr. Gjullin came up from Portland, Oregon, to do surveys of mosquitoes to see what control program might be instituted if mosquitoes were involved. He worked with us but worked separately in the sense that he was on mosquito control and we were trying to find the vectors.

Hughes: I have in my notes the name Izumi.

Reeves: Ernie Izumi was in the laboratory. Ernie was a medical technician doing the virology in the laboratory--the serological work, virus

isolation, things like that. In the middle of the second year of work in 1942, the U.S. government cracked down on Japanese. The first thing we knew was that one day Ernie was sitting there in the lab working, and the next day they came and picked up him and his family and moved them to Tanforan Race Track and later to an Arizona internment camp. So suddenly we didn't have a technician.

Hughes: Did anybody protest?

Reeves: You couldn't protest. The damn U.S. government decided there were not going to be any Japanese within a hundred or two hundred miles of the West Coast, except in Hawaii. It made no difference that Ernie's brother was a lieutenant in the U.S. Navy and had gotten all sorts of decorations for his involvement on our side on Pearl Harbor Day. This happened like this. [snaps fingers] All of a sudden the decision was made, and they just picked up these people, took them out of their houses, and moved them.

Ernie was right in the middle of all these experiments. Fortunately it wasn't the summertime; it happened in the wintertime. Hammon went down to Tanforan Race Track and said, "Look, we can't even figure out what he's doing in the lab." Neither Bill nor I had been in the lab all summer. So we were able to get him brought up from Tanforan with two M.P.'s [military policemen], great big guys. They didn't have him in handcuffs; they might as well have. He had to go over the records with us, because I had to take over the lab. I had to start doing the labwork myself, because we'd collected a lot of specimens from that first summer. Anyway, that was a lot of fun but not an easy time.

Regardless of such problems, we got the project started in Yakima in May 1941. We rented a big old two-story house and converted it into a laboratory. The basement was our laboratory, and we lived upstairs because we were not getting any expense account money for this work. We just got our miserable \$125 a month.

Hughes: Did you worry about infection?

Reeves: Not in those days. I guess we thought we could be heroes. Everything was done on a wooden table that we built out of plywood. All the experimental transmission work, all the virus preparation stuff, was done right on the benchtop. We didn't have any antibodies to protect us and no fancy containment facility. We just did it. It had to be done. That was our job. We didn't have a good insect repellent. The mosquitoes were biting us. All of us eventually developed good antibodies as a result of this. Plus accidents--lab accidents. One day Hammon jammed a forceps

tip covered with infected mouse brain in a finger, and I crushed an ampoule filled with virus in my hand. Red flags went up. All we could do was get an ampoule of horse vaccine and take it after iodine-swabbing our cuts.

Hughes: But nobody got sick?

Reeves: No, not that we know of. Some of my friends still say that I must have had a bad case of encephalitis. [laughter]

We hadn't been there more than two weeks when Hammon came to me one day. He had had a phone call from Dr. Meyer that there was a big polio epidemic in Arizona. He had to go down there, and he couldn't stay and run the program. He was doing all the clinical stuff and so on. He had arranged to get Dr. Fred Bang sent out from the Rockefeller Institute for Medical Research. He wanted some experience in field studies, so they shipped him out and he joined us in Yakima.

Hughes: So he asked for the assignment, essentially?

Reeves: Yes.

#### Reeves Heads the Summer Field Program

Reeves: Meanwhile, I said to Hammon, "Bill, who's going to run this thing? You're going away." He said, "You are." "Me?" I said. "I've never run anything like this before." He said, "You're going to run this one. If you have any serious problems, tell me. Keep me informed of what you're doing and whatever is going on." He took off. I didn't see him until the next month.

Hughes: How did you do?

Reeves: Very well. [laughs] I had a little trouble. Some people didn't think I should be at the reins. I said, "I didn't make the choice. But if I'm given the responsibility, I'm going to do it." So I ran the summer field programs from then on, because Hammon was always off chasing polio or something else, or at some army meeting. He was in and out.

Hughes: Did you run into any problem because you weren't an M.D.?

Reeves: Not really, never have. I treated physicians with respect, and they treated me with respect. No, I've been very, very fortunate, and we'll talk about that more as we go into my academic career.



I've been associated in large part with physicians. I've never had any problem with them. They've always been very considerate of my abilities, I've been very considerate of their abilities, and we've complemented each other. I can truly say that I've never had any problem with the attitude that I'm a technician and they're a professional.

Anyway, that summer we collected every bug we could find that would bite a person. So we collected all the mosquito species that were in the area, and that would mean *Aedes*, *Culex*, *Culiseta*, *Mansonia*, and *Anopheles*. We collected bedbugs, horseflies, and black flies. We even collected some insects like leafhoppers that are supposed to only bite plants. One night I was sitting at a lamp reading, and I suddenly felt this prickle in my arm. I looked, and there was a leafhopper. Apparently they're attracted to the salt when you're sweating, and it was trying to bite me. We collected a bunch of them.

We shipped all these insects back to San Francisco. We were just dumb lucky, because at that time dry ice had come into being, and it was a commonly available refrigerant that was being used for vegetables, frozen foods, and ice cream. Dry ice really hadn't been available on a wide scale before then. We had dry ice and a method of preserving viruses by taking them to the very low temperature of minus 70 degrees celsius. Now we didn't have to test specimens as soon as we collected them and didn't have to maintain them by serial passages in a mouse or guinea pig. We could save specimens and ship them back to San Francisco.

Hughes: Was dry ice commonly used in virology at that point?

Reeves: Just starting. We were on the ground floor. Meanwhile, I'd read a couple of the early papers from the New Jersey Agricultural Experiment Station, where Dr. T. J. Headlee had found that carbon dioxide was an attractant for mosquitoes.<sup>1</sup>

Hughes: Why?

Reeves: When you exhale, you breathe out carbon dioxide. An insect can detect it, and that attracts it to you. They're very choosy about how much carbon dioxide attracts them. So a little bird that puts out twenty-five milliliters of carbon dioxide per minute will attract certain mosquitoes that feed on birds. You put out 250 milliliters on the average, and you'll attract a different

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<sup>1</sup>T. J. Headlee, "New Jersey mosquito problems" in Proceedings of the 20th Annual Meeting of the New Jersey Mosquito Extermination Association, 1941; p. 10.

mosquito. A cow puts out 2,500 and attracts a different mosquito. Some of the mosquitoes can't detect a small amount, so they'll only feed on large animals. Some of the mosquitoes that like small animals are repelled by a high amount of carbon dioxide. That's putting it all in a nutshell.

I started shipping frozen mosquitoes and other insects to the Hooper Foundation, and they were testing them in adult mice. I got a very excited phone call within two weeks of the time we started, saying, "We've got a virus." I said, "Which virus?" "Well, we don't know yet, but we've got at least two mosquito pools that are positive."

Hughes: Was Bea Howitt doing most of that work?

Reeves: No, she was doing none of it. She was frozen out of this project completely.

Hughes: Why was that?

Reeves: Dr. Meyer and Hammon's decision.

Hughes: Yes, I know, but why?

Reeves: Dr. Meyer was in the process of getting her out of the Hooper Foundation.

Hughes: So was there a handful of people doing the labwork then?

Reeves: Hammon was, when he was in town. Ernie Izumi was doing it. We had one girl, I believe it was Margaret Gray, who was working at that time. Over time, we had a large group of technicians that came and went. Ernie was almost the only person who was in the lab at that time. After he left, I frequently had to do both laboratory and field work.

Hughes: Do you know where he learned his techniques?

Reeves: There, at the bench. There was no other place for you to get such training; there were no lecture or laboratory courses in virology at Berkeley or San Francisco in the 1940s.

### Isolating Western and St. Louis Encephalitis Virus

Reeves: These technicians were trained to some degree in microbiology, so they knew how to do bacterial culturing and so on. It was very important, because if you ground up a group of fifty mosquitoes in a suspension and inoculated them intracerebrally into a mouse or a guinea pig, the animals would all die because the bacterial contamination was so heavy that you just couldn't get away with this. So we had to find a way to separate the virus from the bacteria. You couldn't do it by dilution, because if you diluted enough to eliminate the bacteria, you wouldn't get virus. We didn't have any antibiotics at that stage; they hadn't been invented yet. The world didn't know about penicillin or fungicides; they weren't discovered yet.

What we found was that there was a lot of fat in the mosquitoes. We didn't have any high-speed centrifuges. We just had regular lab centrifuges. If you centrifuged mosquito suspensions in a refrigerated centrifuge long enough, the fats would all come to the surface, the heavy stuff would go to the bottom, and the middle layer in the test sample would be pretty sterile except for virus. Most of the time we could take a little pipette and reach down and take out that middle layer, and most of the time we'd get away with it; it would be sterile. Sometimes we knew it wasn't, if the animals died rapidly and bacterial cultures were positive. We had to do bacterial cultures on everything we did, so training in bacteriology was very important. If an animal died, the first thing we did was to culture the brain to see if it had bacteria in it, because if it did, that probably was what killed it. If it didn't, we were in business. We couldn't put the sample through a filter that would remove bacteria, because it also could take out most of the virus. If so, the virus wouldn't have a high enough concentration left to multiply.

But to make a long story short, the San Francisco lab called me in Yakima within another couple of weeks and said, "It's western virus." And shortly after that they called again and said, "Now we have St. Louis virus out," and we were ready to celebrate.

Hughes: These were the first virus isolations from vectors?

Reeves: To our knowledge, the first isolations of any virus ever made from a mosquito. Yellow fever had not been isolated as such from a naturally infected vector. Dengue had never been isolated. None of these other arboviruses had been isolated from a vector.

Hughes: Why do you always stipulate "naturally infected" mosquito?



Reeves: In the laboratory, isolations had been made from experimentally infected mosquitoes. Investigators had fed *Aedes aegypti* on people infected with yellow fever and then fed them on another person. You can say that they isolated the virus from the mosquito, because they had fed it on an infected person and then fed it on a person who wasn't infected and infected him. They could take blood from a person infected with yellow fever or dengue and transfer that blood to another person and infect him. But this hadn't been done for obvious reasons with the encephalitis viruses. This sort of thing had been done experimentally in horses, for instance, feeding mosquitoes on a virus suspension and then feeding them on a horse, or feeding them on a guinea pig, then on a horse, and then maybe even horse to horse. But that's different from naturally infected mosquitoes. What was my next question on that first phone call?--"What mosquito?" They said, "*Culex tarsalis*," and I'd identified all these mosquitoes, so I knew it was right.

Hughes: So that's what you were suspecting?

Reeves: No, I wasn't expecting it at all. It was the last mosquito species I would have guessed, because nobody thought *Culex tarsalis* was of any importance.

Hughes: Did you have any predilections about which mosquito was the vector?

Reeves: None whatsoever. As a matter of fact, if I was heading in any direction, it was that it was one of the species of *Aedes*, the most common mosquitoes that I was collecting. I had found out that *Culex tarsalis* was equally prominent resting in shelters, going to my light trap, or in collections off horses, and that was a new experience. No one had ever worried about collecting this mosquito. If you went to Freeborn's book on mosquitoes,<sup>1</sup> he didn't give *Culex tarsalis* the time of day. As a matter of fact, he didn't even think it bit people. He didn't know what it bit, but he didn't think it bit people because it had never bitten him.

Hughes: Do you think you had been influenced by the dogma about *Aedes*, or were you just going on your observations?

Reeves: My job was to determine if some arthropod was transmitting encephalitis viruses or not. My dogma was that I didn't know which one was; ergo, we were going to look at everything we could collect that could be involved epidemiologically and biologically

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<sup>1</sup>Op. cit.

that would bite people, bite horses, bite other animals in the environment. We weren't even focused on birds at this stage.

Hughes: Had any other virus study ever collected that many different potential vectors?

Reeves: No, not at that time. By the time we got through that summer we had collected 15,610 live arthropods, of which 12,466 were mosquitoes. All were inoculated into mice. We isolated five strains of western and three of St. Louis virus.

Hughes: What problems did you have?

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Reeves: We had encountered a lot of problems to get to this point. I summarized some of them in a paper at the time of my retirement.<sup>1</sup> No one had ever worried about collecting insects wholesale. We realized that there was a probability that the infection rate in any insect species at any one time would not be very high, so we came up with the concept that we wouldn't try to test individuals, because we wanted to test large numbers, and we'd have to make pools. We didn't know what size pool to test. We had to identify every specimen, as we didn't want to have two species in the same pool, because then we wouldn't know where the virus came from. We now say, "Yes, that's obvious." However, a few years later the first isolation of polio from flies was reported in Science by John Paul and company as coming from big black flies, little black flies, and a "miscellaneous object." I asked him, "For god's sakes, what was the object? A stool sample?"

### Collecting Insects

Reeves: We had to work out methods of collecting the insects, and no one had ever worried about it. People had collected them, but sort of haphazardly--find one here, find one there.

Hughes: How did you do it?

Reeves: We just started looking for them. We knew that one way to do it was to collect off animals, so we did a lot of collecting every night. We would go out and collect them off ourselves and off

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<sup>1</sup>Reeves, W. C., The discovery decade of arbovirus research in western North America, 1940-1949. Amer. J. Trop. Med. Hyg. 1987. 37:945-1005.

horses. You say, "Why would you do that?" Because we had nothing better to do. I had horses here and there that I could use and that wouldn't kick me. I even had a big old bull that weighed about two thousand pounds at one barnyard that just loved to have me collect mosquitoes off of him.

Hughes: How did you actually do it?

Reeves: Tommy Aitken and I had developed the mouth aspirator in the lab here. It's just a piece of plastic tubing the diameter of my thumb, and we put a copper screen across the bottom end of it. We put a rubber tube on it that we could hold in our mouth and suck the mosquitoes up. We would get a little horse dandruff and some dirt in our mouths, but at least we had a screen to keep the mosquitoes out. When we went down later to the Central Valley of California where valley fever (coccidioidomycosis) was common, a few people on our field staff became infected using an aspirator. They breathed in a lot of spores. But we don't do that now; we use mechanical aspirators that run on flashlight batteries instead of lung power.

I found that if I went under bridges, into chicken houses, or into barns, I could find mosquitoes sitting on the wall. We found out that if we used light traps they would also come to the light. But we didn't have any traps that would collect them alive. The New Jersey light trap which had been developed to collect salt marsh mosquitoes would attract them to a light, and a fan would shoot them down into a cyanide bottle. The mosquitoes, the moths, the bugs--everything--would go down there, and you'd have a big mess in the bottle, all dead. Well, that didn't help us any. We had to have live mosquitoes.

So Hammon came to me before we went out the second year. He said, "How are we going to collect these bugs?" I said, "I need a light trap. I need a trap that will collect them alive." He said, "What's a light trap?" I said, "None of the type I want has ever been built." "Well, how would you build it?" We were having lunch, and I took a paper napkin and demonstrated. "Well, I'd have a cone like this, and I'd have a light in there, and then I'd have the insects go through this cone into a box and have a fan in the box that pulls the air so they don't go through the fan. They'd be caught in this box, and they couldn't get out as long as the fan was working. Then I've got to be able to get in there and get them out alive." That's the basic idea. I was thinking of something I could carry under my arm, and with a sleeve I could put the aspirator in to get them out.

A few weeks later, Hammon called and said, "We've got your first trap made. I want you to come over and look at it." I



hadn't drawn anything to scale. I went over, and here's the "Hooper Folly." The damn thing is mounted on a trailer, and it's as big as a small house trailer. [laughter] They had a canvas sleeve that was around the end of the funnel, and you had to pull the funnel out of the sleeve. Then you'd crawl in through the sleeve, get into the box and collect the mosquitoes. It worked; we got mosquitoes in there and a lot of other stuff as well. You were in there with the mosquitoes, and they were biting the hell out of you because they were hungry and we didn't have an effective repellent to use.

Then I realized that I wanted carbon dioxide in addition to light in this trap. However, if I put the dry ice in the funnel, the carbon dioxide went out the back end. That didn't work, because then the mosquitoes would all go to the back and not in the trap. So I got a long piece of stovepipe and put that at the back end and brought the carbon dioxide back out at the front end. It worked; it doubled my collection counts.

At one point Hammon had the bright idea that we should use monkeys instead of CO<sub>2</sub>. A carryover from his African days, I guess. He sent two monkeys in a cage to Yakima. You could smell them a mile away. The boy I had tending them let them out one day, and they took to a big cherry tree. End of experiment. I caught them and shipped them back to Hooper.

I took that damn trailer out in the field and used it for a whole summer. But then we modified and made a smaller trap that was more portable. I couldn't carry it under my arm, but it was smaller. Now they're miniaturized.

Hughes: With the CO<sub>2</sub> attachment?

Reeves: With light and/or carbon dioxide. Actually, in most of our work we do now, we don't use the light at all. We just use the carbon dioxide, and that way we don't get all the other bugs; only the mosquitoes we want come in. We also lose the male mosquitoes because they won't go to the carbon dioxide, because they don't take blood. Only the females take blood. The females have to have blood to lay eggs unless they're autogenous.

Anyway, we had to develop these techniques. By today's sampling standards, we hadn't sampled the population at all, but we got viruses. Today we would collect tens of thousands of mosquitoes of each species to assure we were not missing a virus that is there. If we obtain a sample of 10,000, we can apply statistical tests to evaluate the infection rate in that mosquito population. We also had collected animal bloods, and we found indeed that the viruses were infecting a very wide spectrum of

domestic and wild animals, mostly birds and not the small rodents, but also the very large animals like horses or cattle.

So that gave us the idea by the end of the first summer that *Culex tarsalis* was a mosquito that we were interested in, and the other mosquitoes weren't involved significantly with infection. The findings still had to be confirmed and expanded. It looked like this mosquito, that hadn't been thought to be of any importance by anybody, suddenly was important, but we had not shown that it could transmit the virus. All we had shown was that it was infected. We recognized at that stage that we didn't know whether it could transmit the virus by bite or not. But what we had learned was a big step forward. We had the first paper in Science within two months of the first isolation.<sup>1</sup> Today the journal would still be arguing about whether they wanted the paper or not, unless it was about the AIDS virus; then they would take it.

Hughes: Was it somewhat a similar situation?

Reeves: To some degree, yes, except that AIDS was never seriously thought to be transmitted by arthropods, although some people thought it might be.

Hughes: No, I mean that at the time the interest in encephalitis was comparable to the present interest in AIDS. Wasn't that why your paper was published so quickly?

Reeves: Yes. It was a major advance in the field of arthropod-transmitted diseases. It explained how a group of diseases were transmitted and a possible approach to control. It was big stuff and very exciting.

### Colonizing *Culex tarsalis*

Reeves: By the end of that first summer we knew we could begin to focus some of our interest and could evolve the questions to be answered. The first problem that we faced was that no one had ever worked with this mosquito before. We didn't know how to handle it in the laboratory; we didn't know whether it could be colonized. The usual first step in these sorts of studies, if you

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<sup>1</sup>W. McD. Hammon, W. C. Reeves, B. Brookman, E. M. Izumi, C. M. Gjullin, Isolation of the viruses of western equine and St. Louis encephalitis from *Culex tarsalis* mosquitoes. Science 1941, 94: 328-330.

are going to do this research, is that you have to colonize the mosquito so you can study it. Fortunately, we didn't go that route, because it was twelve years later before we finally colonized *Culex tarsalis*.

Hughes: Why did it take so long?

Reeves: We just couldn't get them to breed in the laboratory. They wouldn't colonize.

Hughes: What eventually did the trick?

Reeves: We didn't even do it. A fellow by the name of Jim Brennan at the Rocky Mountain Laboratory in Montana was also working on these viruses, and he was interested in trying to colonize this mosquito. We had built insectaries that were three times the size of this room, because we thought the problem was that mosquitoes wouldn't mate in a small space, and that was why they weren't colonizing. So we'd provide plenty of room for the males to swarm and the females to do their thing. We could get the things to colonize in the summertime. We'd have a pond of water in there, they'd lay eggs, and we had chickens in there for them to bite and so on. But the minute winter came, they disappeared. We could colonize them on a porch. I'd have a smaller cage out there, about three feet by three feet, and I'd get them to lay eggs, but the moment winter came, I wouldn't get any more eggs; they'd disappear.

I don't really know how Jim got onto this, but he did. He found that what triggered mating of these mosquitoes was the length of daylight versus dark, and mating took place right at dusk. If you had the daylight period long enough and the nighttime period short enough, they would mate, and they would colonize. You had to regulate the light as happens in nature in the summertime. In the wintertime, the daylight hours decrease, and the mosquitoes will stop mating because they go into overwintering diapause to get through the winter. If they didn't do that, they wouldn't make it, because they couldn't resist the cold temperatures. They have to go into this overwintering state. Jim discovered this more or less by accident, and he called us and told us what he'd found, and the minute we did it, bingo, we also were in business.

To go back, we knew which mosquito we wanted to study, but I was too impatient to worry about colonizing that mosquito. I wanted to know, can that mosquito get infected with a virus, and can it transmit? So I was perfectly willing to take mosquitoes from nature that were out there ready to bite, bring them into the laboratory, feed them on virus, and see if they transmitted it.



People said, "What if they're already infected?" I said, "Fine. All I want to know is if they can transmit it by their bite. We'll worry about the nice stuff later."

We also knew by this time that we were interested in relatively small birds as hosts, not because they were sick but because we were getting a lot of them with antibody.

That first winter we worked out a source of virus, which was mouse brain. That could be frozen, and the virus would persist in this state. We did some preliminary experiments and showed that we could work with young chicks, pigeons, doves, or ducks. If we infected them, they would have a lot of virus in their bloodstreams. So here we had a potential source and could infect mosquitoes by feeding them on blood, or you just put the virus in some defibrinated blood of any type and saturated a cotton pledget. They'd feed on it if you had a little sugar in it. So we had a model that we could work with in the second summer to see if we could get infection and transmission and could extend the business of what proportion of the mosquito population was infected. We also could eliminate the other species even further by further testing and so on. We went into the second summer with the idea of having a really intensive field evaluation of the ability of the mosquito to be infected and to transmit the virus into birds.

#### Encephalitis in Texas, 1941

Reeves: We got interrupted, meanwhile, by an epidemic in 1941 in the lower Rio Grande Valley of Texas, in which eastern, western, and St. Louis virus all were active. This was one of the reasons that Hammon pulled out of Yakima in that first summer of 1941, because he had to go down there and investigate that epidemic. So in 1942 the Texas State Health Department decided it would like to have that area investigated, and we went down to San Benito in April. It was one of the cheapest control programs the state could have had, because nothing happened. They had a drought, there were hardly any mosquitoes around, there were no cases, there was no virus activity. This was the first demonstration of "Reeves' rule number 1": call us in to do a study, and the disease and viruses will disappear.

Nevertheless, I set up a lab down there in April, 1942, and did the first transmission work in Texas instead of in Yakima. I couldn't get the right mosquitoes, though. I couldn't get very many *Culex tarsalis*, because they weren't occurring there. It

turned out that *Culex tarsalis* is a wintertime mosquito in the lower Rio Grande Valley, not a summertime mosquito. It's too miserable there in the summer for almost anything to live. But I was able to transmit virus with a couple of species of the local mosquitoes and get the preliminary work on methodology done so I could follow up intensively when we got back to Yakima. We showed very quickly that *Culex tarsalis* could be infected with and transmit western and St. Louis viruses. *Culex pipiens* also was a common mosquito in Yakima. So I did experiments with them, and I showed that *Culex pipiens* also could transmit St. Louis virus but not western virus. This was the mosquito that Lumsden had suspected previously was important in the St. Louis outbreak of 1933.

Hughes: Did that revive his work?

Reeves: No. He was gone. I never met Lumsden. I don't know whether he was dead or what happened. I met Leake and Musson. And Hal Chope was the health officer of San Mateo County in California a few years later, so I knew him well.

So at the end of that time we gave our first big paper at the American Public Health Association meeting on the unraveling of all these problems.<sup>1</sup> It was a very heady experience to get on the train after buying my first overcoat and felt hat so that I'd look proper when I got to St. Louis. Hammon and I wrote the paper while going there on the train because we had plenty of time. We had about two or three days getting there. We had a little portable typewriter. We got up to great acclamation and gave our first presentation on what was known about western and St. Louis at that time.

Hughes: How did people respond?

Reeves: Enthusiastically. The interesting thing is that Dr. Leake, of Leake, Musson, and Chope, was in the audience. He was a pretty bombastic character. So he gets up as soon as we're through with the paper and grabs the microphone and commends us for cleaning up the loose ends of the work that he'd done in St. Louis and so on and so forth. We decided to ignore his comments, as his report had said mosquitoes were not the vector.

That paper was given in March '43, right after the second summer of field studies.

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<sup>1</sup>W. McD. Hammon, W. C. Reeves, and M. Gray. Mosquito vectors and inapparent animal reservoirs of St. Louis and western equine encephalitis viruses. American Journal of Public Health 1943; 33: 201-207.

### Laboratory Procedures

Hughes: My notes say that you used the precipitin test to identify blood meals from mosquitoes.

Reeves: Yes. Actually, it was interesting, because as I said earlier, Dr. Fred Bang had joined us and was doing the medical aspects of a lot of the work during 1941. As a medical student, Fred had gone to Reelfoot Lake as a special field summer experience on *Anopheles* mosquitoes as malaria vectors. Reelfoot Lake was where they did some of the earlier studies on malaria in humans in the U.S. It was a TVA [Tennessee Valley Authority] project in Tennessee. They were trying to determine if these dam developments were having a positive or negative effect on malaria transmission. He had gotten some training with Mark Boyd, the same person that Hammon had trained with in Florida.

They had a very crude test in which they could inoculate animal blood--say, human blood--into a rabbit, and the rabbit would develop antibodies to the human blood. They could take the serum from that sensitized rabbit and put it into a capillary tube below a suspension of a mosquito blood meal in saline they had already put in the capillary tube. So they had a layer of the mosquito blood meal above the serum. If the blood in the mosquito was the same type as in the serum, you'd get a precipitin ring between the interface of the two fluids and had identified the blood meal.

The trouble was that they'd only developed this test for a very few animals, because malariologists were interested in the question, are *Anopheles* mosquitoes feeding on people? So it was very important to have the human antiserum in their tests, but they couldn't care less if the mosquitoes had fed on a bird or a chicken or anything else, as that wasn't a source for human malaria. Fred had some of these immune sera, so we put together a little paper on the source of blood in the engorged *Culex* that I had collected in Yakima.<sup>1</sup>

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<sup>1</sup>F. B. Bang, W. C. Reeves. Mosquitoes and encephalitis in the Yakima Valley, Washington: III. Feeding habits of *Culex tarsalis* Coq., a mosquito host of the viruses of western equine and St. Louis encephalitis. Journal of Infectious Diseases 1942; 70: 273-274.



He had antisera to cow, horse, dog, chicken, human, pig, sheep, and rabbit blood. But he only had one type of bird in there. And we only had fifty-seven engorged mosquitoes, which seemed like a lot then but not now. Well, four of the *Culex tarsalis* had fed on chickens, and all the rest had fed on large mammals, which probably reflected where they were collected or how they were collected. I don't even remember how they were collected because we weren't that sophisticated then. But the numbers didn't give us much of an indication that this mosquito really prefers birds. I think it's the only series of specimens we've ever had that had that low rate of only four out of fifty-seven specimens fed on birds. This would be 7 percent feeding on birds, whereas in most of our work we have 50, 70, even 90 percent feeding on birds.

This work was done in '41. It was at least 1958 before we brought Dr. Constantine Tempelis into the project. He had the sophistication to really separate the blood from different bird species. We had to develop completely new methods.<sup>1</sup> If you inoculate a rabbit with chicken blood, the antiserum you get will react to any bird species, because all birds share antigens, and a rabbit is so distantly related to birds that it reacts to every antigen that's in there. So we had to make our antisera for birds in a bird, like a chicken, and then they wouldn't develop the reactions to their own antigens, and they would only react and make antibodies to whatever was unique for the other bird species and that was different from what chickens had. In that way we could develop a more specific method. But it took a lot of years to get smart enough to do that.

### Simultaneous Discoveries

Hughes: What about the amazing coincidence of the two other isolations of arboviruses that occurred within a short time of yours?

Reeves: Well, I didn't discover that until very recently, and I forget which paper I put that in.

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<sup>1</sup>C. H. Tempelis, M. L. Roderick. Passive hemagglutination inhibition technique for the identification of arthropod blood meals. American Journal of Tropical Medicine and Hygiene 1972; 21: 238-245.

Hughes: Nineteen eighty-seven.<sup>1</sup>

Reeves: I don't know how I found out; I guess I just dug through the literature and discovered that in the same summer of 1941, and a few days earlier than we, Mitamura and co-workers in Japan had isolated Japanese B encephalitis virus from mosquitoes.<sup>2</sup> He'd done this during the war, and we didn't have that literature available to us until way after the war. It was buried in a Japanese journal that he'd isolated Japanese B virus from *Culex tritaeniorhynchus*. Then there was another worker in Africa. I forget his name now.

Hughes: [Alexander F.] Mahaffy and associates.

Reeves: He'd isolated yellow fever virus from mosquitoes within the same week that we had, which had not been done for the virus before.<sup>3</sup> As a matter of fact, to show you how buried that information was: Something had come up about whether our virus isolations were the first that had ever been made from mosquitoes. I wrote to Dr. Fred Soper, who was the key person in the Rockefeller Foundation yellow fever work, and I asked him, "Is this the first isolation of an arbovirus from a mosquito, or have there been a lot of isolations of yellow fever virus that I never heard of?" Fred wrote me a letter back that said, "Yours is first. We've never isolated yellow fever virus, and no one ever has, from a mosquito." That was in the fifties, say. He'd missed Mahaffy's isolation.

We never worried that much about priorities in this area, but I guess that actually Mitamura probably would be the person who made the first arbovirus isolations ever from mosquitoes in nature.

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<sup>1</sup>W. C. Reeves. The discovery decade of arbovirus research in western North America, 1940-4. American Journal of Tropical Medicine and Hygiene 1987; 37(3) Supplement: 94S-100S.

<sup>2</sup>T. Mitamura, M. Kitaoka, M. Imai. Seasonal occurrence of mosquito and its infectivity of Japanese encephalitis virus in Okayama City, 1942: Relationship between the grade of epidemic and the infectivity of mosquito. Japanese Medical Journal 1950; 3:149-159.

<sup>3</sup>A. F. Mahaffy, K. C. Smithburn, H. R. Jacobs, J. D. Gillett. Yellow fever in western Uganda. Trans. R. Soc. Tropical Medicine and Hygiene 1942; 36: 9-20.

Evolution of the Term Arbovirus##

[Interview 3: December 18, 1990]

Hughes: Dr. Reeves, who is responsible for introducing the term arthropod-borne virus?

Reeves: It was an educational process, how that term arose, because when Dr. Hammon came to the university in 1940, he didn't know anything about mosquitoes for practical purposes, in fact didn't know a lot about arthropods. You have to realize that arthropods are a large phylum of animals which includes insects, spiders, scorpions, and crayfish. Arthropods is the descriptive term of this general group, and all of the vectors of arboviruses or arthropod-borne viruses actually fall in that general group of animals. So it was part of the education of my boss, which as an entomologist I felt duty-bound to do, that he understand the entomological aspects of our research.

The only vectors that we found involved with any of the viruses we worked with, whether it was western or St. Louis or later California encephalitis virus, were mosquitoes. So we called them mosquito-borne encephalitides, which described mosquitoes as carrying encephalitis viruses.

Then it became obvious, as the field developed in other parts of the world, that there were very many important diseases carried by ticks that also were virus diseases. Some of these also caused encephalitis, so then we began to call them arthropod-borne virus encephalitides. As the field developed even further--now we're up to some five hundred viruses in the world that fall in this group--we found some were carried by mosquitoes, some by ticks, and some by other blood-sucking arthropods of different types. So the term evolved from mosquito-borne encephalitis, which described the western and St. Louis we were working with, to arthropod-borne encephalitis.

This was too many words and too much of a mouthful, plus we wanted some sort of an abbreviated term that would be acceptable worldwide. So originally it was presented as arbor viruses, but when we hit the international audience at WHO meetings, people said, "What does it have to do with trees?" Because "arbor" to them meant "trees." So we shortened it to "arbo" for "arthropod-borne," and that satisfied them. By a process of derivation and pleasing the international audience so they would accept the terminology, it had gone from mosquito-borne encephalitis to arthropod-borne encephalitis to arthropod-borne viruses to arbor viruses and now to arboviruses. So that's the sequence.



Hughes: It's the American Committee on Arthropod-Borne Viruses that is responsible for the final name?

Reeves: The WHO's responsible for the acceptance of that final name, although it was the arbovirus committee that was really pushing the name arthropod-borne viruses in the early 1960s. The term "arthropod-borne virus" was accepted by the WHO study group that met in Geneva in 1960. I was a member of the secretariat for that meeting and report.<sup>1</sup> Certainly the term arbovirus was established and accepted shortly after that.

#### More on William McDowell Hammon

Reeves: We discussed Dr. Hammon before. In some ways, he was an enigma to me, in the sense that here was a person who was trained and ordained as a minister, a missionary in Africa, and who had then gone into medicine, and he was now working with biological systems. Sometimes his religious and scientific creeds didn't exactly come together, which meant we had many very interesting, long conversations as we drove between San Francisco and Yakima, Washington, or San Francisco and Bakersfield.

The result of this combined training was that here was a man who had extremely high standards of honesty and morality and everything else that went with his religious background. The general philosophy that he had from religion also was applied to his science, so that when someone would do something in the science area that he thought was not quite honest, he would say so very loudly, which didn't exactly make him popular with some unmentioned people whose names are well known in science today.

But at the same time, he was an extremely well-trained scientist and a very kind individual, and he was a person who in my experience never took advantage of anyone who worked in any sense with him, from the lowest level of janitorial services or the laboratory technician and dishwasher to the top-notch scientist that he worked with on international committees. He was equally considerate and a gentleman with all of them at all times.

He was a very demanding scientist and had extremely high standards of scientific work. It was a fetish with him to have

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<sup>1</sup>Anonymous. Arthropod-borne viruses. WHO Technical Report Series No. 219, 1961, WHO Geneva, pp. 1-68.

adequately controlled experiments at every opportunity. Sometimes some people accused him of having too many controls in his studies. He later conducted the polio gamma globulin experiments with the National Foundation for Infantile Paralysis, which he carried out in Texas, to show that giving a person polio antibodies would minimize the chances of a person getting this infection in the face of an epidemic. It really was the first step in the National Foundation's drive to develop a vaccine. Gamma globulin was available, and it was a way of giving immunity to people by inoculation.

But when he did those studies, he insisted upon having the study very accurately controlled so that half of the people who thought they were getting inoculated with gamma globulin were actually getting gelatin without antibody. People were told in advance when they volunteered for the study that half of them would be in the immune serum group and half would be getting something that had no antibodies in it. Hammon explained to them carefully why this was necessary to prove whether gamma globulin was effective or not effective.

That was a double-blind study, so everything was coded. The people who inoculated the material had no idea what they were giving; the people who got it had no idea, and at the time it was done, Dr. Hammon had no idea. Those codes were in New York in a safe at the National Foundation.

Hughes: Was that protocol that you just described well established? I assume you're talking about the fifties.

Reeves: I'm talking about the late forties and early fifties.

Hughes: Were double-blind studies well established then?

Reeves: Not to the extent they are today. It was one of the first double-blind trials that was done on an extensive basis. Science had developed to an extent that this setup was soon going to be expected and not just something somebody did because he thought it was right; it actually became a part of the standard that was expected.

Hughes: Was Hammon given a protocol?

Reeves: No. He developed it. He was completely responsible for all aspects of the study. The National Foundation said, "You are the person who has been interested in seeing if gamma globulin would provide immunity. We want you to be responsible. We'll provide the budget; we'll provide the staff; you design the study and tell us how it has to be done. You're the scientist."

At the same time, while they were very careful to record how many people got poliomyelitis in this study, Hammon was also recording how many were getting some of the other common childhood diseases. So he recorded how many people got measles, chicken pox, and other things, which wasn't the purpose of the study, but it was his concept that you had to have a large enough body of people involved in the study to have statistical significance. The fact that there were maybe a few more cases of polio in the gelatin group than in the gamma globulin group was important. However, if it had happened for the other diseases or the reverse took place, it would raise serious questions.

Well, I am not privy to all of the information, but if my memory is correct, the National Foundation staff got pretty excited early in this epidemic in Texas. It looked like maybe the immune serum was working, so they wanted to have a press announcement about this. Hammon said, "Not until I see the data, because I want to see what happened to measles and the other diseases." And indeed, it seemed to show at that stage, when they initially wanted to break the news, that the immune serum was having an adverse effect on the risk of getting those other diseases. It really wasn't; it was just a matter of the numbers not being big enough to give it the power that was necessary. This I think epitomized him as a scientist, that he stuck to the protocol until it was worked out completely.

Again, this came to the forefront when he became extremely interested in developing an attenuated virus vaccine for Japanese B encephalitis. This was part of his work with the Armed Forces Epidemiological Board Commission on Virus Diseases. He'd been given a considerable amount of money to do research on this and to see if he could indeed select attenuated strains of Japanese B encephalitis virus, which was an epidemic disease in Asia and had been of some importance to our troops in Okinawa and eventually in Japan and other areas of Asia. There was a formalin-killed vaccine that Albert Sabin had developed which had not been very effective. The idea was that an attenuated vaccine, which would be along the lines of the yellow fever 17D vaccine or the vaccines that were developed later for measles and polio, would be the answer to the problem.

Well, he worked very hard on this and actually developed strains of the virus by selection that no longer would produce disease in animals. So it seemed like this might be a very effective vaccine. But unfortunately he also found that these attenuated strains were not good immunizing agents. So once that became obvious, and he'd worked on this for several years, he just stood up at one of the virus commission meetings, gave his report



and said, "I'm convinced this isn't the way to approach this problem. Rather than to string it out longer, I just want to return the money that I haven't used. I'm going to withdraw from this research." This is one of the few times in science where a person gave back money that they'd been given to do research. Usually they find some way to finish spending it rather than give any back.

I learned my object lesson from that. Errol W. Mauchlan, Assistant Chancellor for Budget and Planning, who for years handled the business office of the university as far as funds were concerned, laughs when he sees me and says, "You're the only person on the Berkeley staff that I can remember who has ever given me any money back that he didn't spend on his research." That's a lesson I learned from Bill Hammon.

Hughes: Why did you give money back?

Reeves: I don't remember the details of it. I had some state money that I found shouldn't be spent the way I wanted, so I gave it back to him. He thought that was humorous. I didn't think there was any alternative.

Hughes: It sounds as though Hammon strayed away from the encephalitides.

Reeves: As I say, he left here in 1949 and went to Pittsburgh, where a new school of public health was being formed. Dr. Thomas Parran, who had been the surgeon general for the U.S. Public Health Service, had retired and was made dean of that school. He recruited as faculty almost entirely people from the U.S. Public Health Service who were retirees or were willing to leave the Public Health Service. He recruited Hammon to head up the epidemiology program. For practical purposes, Hammon was the only faculty person there at that time who had any experience in teaching. Of course, by that time he'd had ten years of academic experience at San Francisco and Berkeley, including being the dean of the School of Public Health for a short period in the mid 1940s.

So he went to Pittsburgh, and he took Gladys Sather and Al Rudnick with him, who were involved in his research program. He took all the research grant and contract funds with him for dengue and polio but left the small encephalitis research grant with me. By this time his research interests had changed from encephalitis and polio, and he'd become very involved in studies of dengue, hemorrhagic fever and shock syndrome. As I said earlier, he was the first person to discover and describe the relationship of dengue viruses to hemorrhagic fever and shock syndrome. He was in the Philippines actually doing a study on poliomyelitis, because

it was a threat to the health of the service people we had there at Clark Air Force Base.

He was called on a consultation to a hospital to see children who were dying of hemorrhage and shock syndrome, and he recognized and obtained the necessary materials to prove that this was actually being caused by dengue fever viruses. Up to that time we knew that there was some degree of hemorrhage with dengue fever. The textbooks all said that people would get dengue and wish they'd die because they felt so miserable, but they never did. It makes a severe case of influenza sometimes look mild in comparison. It causes a lot of neurological reaction and joint pain. They also call it break-bone or dandy fever.

So Hammon had changed his research interest largely to dengue, which led to a lot of research in the Philippines and then later in Thailand, where hemorrhagic fever and shock syndrome were merged as a complication of dengue. So he's really the person who opened up that field, which was then taken over by the army in their laboratory in Bangkok. They still have a laboratory there and run it as an overseas operation.

He also, by some method that I've never quite understood, became interested in hepatitis virus and did some work on a hepatitis virus that occurs in ducks, which didn't turn out to be very productive. He also became involved in cancer research. But he never really put his heart, soul, and intellect into full operation in those fields.

Hughes: You mean the viral cause of cancer?

Reeves: Yes. He never really focused on that. He was approaching retirement by the time he got into these fields. He finished up the dengue work and finished up the Japanese B encephalitis work. Then when he retired, he moved to the Tampa Bay area, near Saint Petersburg, and he never did any more research. He stopped his memberships and activities in scientific organizations, stopped his subscriptions to scientific journals, and devoted the majority of his time to his church and elderly relatives that he had in Florida. He also spent a lot of time fishing, but he really devoted most of his time and his efforts and interest to church activities.

More on the Yakima Study

Hughes: What sort of a relationship did the two of you have in Yakima?

Reeves: We had a very close relationship in the sense that we worked side by side from daybreak until after dark every day except Sunday. Sunday he went to church. Actually, it was the only time that we were separated, because we lived in the same places and ate our meals together. His daily schedule was to go to the hospitals in the Yakima area to find any encephalitis cases that were being admitted, to get the necessary diagnostic materials on them, and to work with the local physicians. My job was to go out and chase bugs. He and other physicians would go out with me at times because they found it interesting. A lot of physicians find this very interesting, much more interesting in some ways than sick people are every day--to go out and see what bugs do and to learn something about them, which he did. In turn, he would take me to the hospital to see cases, because his attitude was that I ought to know how dangerous these diseases were and as much as I could about them if I was going to be working on them.

Hughes: But basically you were in charge of the field studies, and he was in charge of the medical?

Reeves: He was in charge of the whole program, except that after we'd been there a very short period of time, he got a call from the National Foundation [for Infantile Paralysis] that there was a big epidemic of polio in Japanese people who were in the war camps, and he had to go down to Arizona and investigate that. Then he was off on some National Foundation series of talks to raise funds. As I think I described in an earlier session, that's when he said, "You're in charge," and he left. And then Dr. Bang came from the Rockefeller group to spend the rest of the summer with us to continue chasing cases down.

**Differential Diagnosis**

Reeves: It was a very interesting experience to be taken in to see the cases of encephalitis and to see them do spinal taps or autopsies on them, because this was something that wasn't part of my entomological training. It also was not only interesting but in some ways profitable for him sometimes to have me along. I remember one time we went in to see this lady who was in the Yakima County Hospital, and he was doing a spinal tap on her. I picked up her chart and started looking at it, and I recognized



that there was something peculiar about the fever curve this lady had, that it was spiking about every twenty-four hours. I looked at that, and it was a classical picture you see in the malaria textbooks for falciparum malaria, which is a really dangerous type of malaria that can affect the brain but which is unusual in the United States.

So I asked him just curiously, "How do you know this lady doesn't have malaria?" My question was asked out of ignorance, not out of intelligence. He looked at me and said, "You've got to be crazy. There's no malaria here." I said, "Yes, but look at this fever chart." She was a new case, and he hadn't really looked at the chart closely. To make a long story short, nobody in the hospital knew how to diagnose malaria, except he did and I did. So we got some slides and made some blood smears, and this lady was heavily infected with falciparum malaria, which is the most dangerous type, and she'd never been out of the Yakima Valley. So obviously somebody, probably a migratory worker, had brought this infection in. We had plenty of *Anopheles* mosquitoes there, and she'd been infected. If this diagnosis hadn't been made--and the only drug they had to prescribe was quinine--she would probably have been dead. So that sort of an experience always was interesting for both of us.

Similarly, I asked about another case, how he could tell a diabetic coma case from encephalitis. It turned out this young girl was an unknown diabetic, and she was in a diabetic coma. It was not encephalitis, and with the correct treatment she survived.

When the disease that you're studying is epidemic, it becomes a very popular diagnosis by the clinicians. It's the first thing they suspect, if it fits at all; it's that until proven otherwise. It's not easy to separate these viruses, because you can't just look at the cases clinically and say, "This is western, this is St. Louis, this is polio, this is something else." You have to use differential diagnosis, which means you have some lab support.

Hughes: That was one of the points of the field study, was it not?

Reeves: Yes, it was to differentiate these various causes.

Hughes: And you did indeed find that the diseases had been thrown into a grab bag.

Reeves: That's right. As a matter of fact, they still are. We've had current experiences with St. Louis encephalitis in California in 1989 when an outbreak in Kern County was called aseptic meningitis. All that meant was that it was caused by something

that wasn't bacterial. It turned out that a majority of the cases were St. Louis encephalitis.

But it takes time to make the diagnosis, and the specimens have to be submitted to a diagnostic laboratory that's capable of doing the separation, and the physician has to ask for that separation to be done. You can't just collect a sample and send it to a lab and say, "Test for a virus."

Hughes: In the forties, that laboratory was the Hooper, wasn't it?

Reeves: In the early forties we had the only laboratory in the western part of the United States that was doing diagnostic work to try and separate central nervous system viral diseases and had the laboratory know-how to do that with a variety of virus diseases-- polio, encephalitis, rabies, et cetera.

Hughes: And the other one was the Rockefeller?

Reeves: The Rockefeller Foundation really wasn't that much involved in trying to diagnose cases, except yellow fever, malaria, and hookworm. This was back about 1940. I would say it would be the mid-1940s before other laboratories had begun to pick up the methodologies that had been developed, some at the Rockefeller Foundation, some here. The Rockefeller Foundation had an international group [the International Health Division] that was more interested in the virus diseases outside of the United States, yellow fever in particular. So their methodologies were very usable in the work we were doing and vice versa. But there just were not diagnostic laboratories that were set up to do this until the methodology had been worked out.

Hughes: Is it possible to distinguish what Dr. Hammon contributed to the study of encephalitis and what Dr. Reeves contributed to the study?

Reeves: Frankly, we never tried to differentiate. If you look at the literature, you'll see that we felt we were working on these things as a team. I think if there's one thing that epitomized all the research that I'll be talking about, it's that it's always been a team effort. As we get into the next period of time when we were working here in California, I think we'll bring out time and again how it required putting together a team that represented the different sciences that had contributions to make at that time, and that it was very rarely that an individual had all the ideas or the knowledge of methodology to carry out the studies.

One of the interesting things about animal infections transmissible to man, that we call zoonoses (and that include the

arboviruses), is that most of them involve a variety of different vectors and different hosts, and man is an accident in the infection and is not important as a source of infection. You have to have people from a number of scientific fields involved in studies in order to get anywhere. As a matter of fact, all you have to do is to look at some of our earlier teams, and what did we have? We had physicians, entomologists, vertebrate zoologists, nurses, and virologists. We had to have all these different people. You just couldn't get the job done without that large body of people working together as a team.

They all have to have credit, too. One of the things in our studies that I think was typical was that we always insisted on giving credit to all of the scientists who were involved. We didn't use them as peons. They had a real contribution to make in the thinking, and then they carried on with the studies. The really tough stuff was right out there in the field.

Hughes: What is the history of the multispecialist team?

Reeves: I can't really answer that question. It seems to me, and I guess I'm biased probably, that the multidiscipline team that involves fieldwork developed just about this time in the early 1940s. If you look at the earlier studies that had been done on diseases of this type and on other diseases, it was almost all individual effort, going out and doing what we call shoe-leather epidemiology. They wore out their shoes going from case to case getting information. When you got into diseases like measles or smallpox, you didn't need to be involved with wildlife cycles or the mosquito vector cycles and so on. If you go into the early histories, even of malaria, the people who originally did a very high proportion of the work were physicians. Then they began to bring the entomologists into the operation and recognized the contributions they could make with their special training. Some of the early studies done on malaria and on yellow fever would have benefited a great deal if there had been people available who were trained in entomology who really knew something about these insects.

#### Parallel Research on Yellow Fever and Malaria

Hughes: I have a quote from Dr. Meyer's oral history. He says, "All that work," meaning your work and Dr. Hammon's work, "was done up in Yakima, one of the most brilliant studies ever undertaken, although it had its model in the work which was done by the Rockefeller in jungle yellow fever when they also showed that



other mosquitoes than merely the *Stegomyia* mosquito can transmit yellow fever."<sup>1</sup> Now, was that a conscious model?

Reeves: We were very conscious of what the Rockefeller had done in yellow fever. There were studies in Africa which had involved a variety of species of mosquitoes, and again, it's interesting that the people who got most of the attention and most of the credit for the work in Africa on yellow fever were young physicians. There were some entomologists who became involved there, but it wasn't until about the time that we were also bringing entomology fully into bearing on such problems that they began to bring entomologists in strongly.

What the Rockefeller Foundation had done was they had started a program to eradicate yellow fever from the New World. The reason they thought they could do that was that they thought it was limited to port cities and that it was a disease strictly of humans and was being transmitted by a domestic mosquito, *Aedes aegypti*, which fed very frequently on man. This was the classical epidemic yellow fever that was known in the Americas.

Now, this was fine, and they had a big program to eradicate that mosquito and to eradicate that disease. They used entomologists in those programs, but to a great extent as technicians rather than as people who were leading the studies. This again is what had happened in the early studies done by the army on yellow fever in Cuba and then later in [William C.] Gorgas's work in Cuba. It was almost all based on this one mosquito. It was a simple problem; all you had to do was use a military-type operation where you had the power to go to any house and get rid of that mosquito by controlling the water that was available for it to breed in. Oiling of swamps also was done when necessary for malaria control. But you had to have that military power. It was a matter that once you knew the simplicity of the system, it was a pretty straightforward thing to do if you had that power.

Dr. Fred L. Soper was in charge of the program for the Rockefeller Foundation in Brazil and other parts of Latin America. Some of the other people who worked there were Drs. Richard Taylor and Austin Kerr, who will be mentioned later, and M. Jorge Boshell, who was a physician with a lot of experience in entomology. Everything was going fine, and then they began to get reports of yellow fever cases from inland in Brazil. As a matter of fact, Dr. [Edwin H.] Lennette, whom you've done an oral history on, was also involved, mostly in the laboratory aspects of the

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<sup>1</sup>Karl F. Meyer, Medical Research and Public Health, p. 220.

work there.<sup>1</sup> And many other names that are very well known in science today were involved, such as Hilary Koprowski, Jordi Casals, John P. Fox, and others.

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Reeves: The problem was that yellow fever cases started to be reported from inland Brazil, where the yellow fever mosquito didn't occur. That's when they discovered that there was a hidden cycle in the primates--in monkeys--and that there was another group of mosquitoes called *Hemagogus* that lived in the treetops where water collected in bromeliads and in treeholes that formed a habitat in the canopy of the jungle. So the mosquito lived up there with the monkeys, and they never, for practical purposes, came down to the ground level, and they were transmitting and maintaining yellow fever. People went into the forest to cut trees down, and when the trees fell, down came the mosquitoes with the trees, and they would bite the people who were on the ground. They also recognized that monkeys were dying in the jungle, particularly the howler monkeys, which indicated virus was active.

This opened up a whole new bag of things to be looked at in the field. That's when they began to bring in entomologists, because these mosquitoes had never been studied before; they were not thought to be of any importance at all before. It was the same problem we had when we proved *Culex tarsalis* was a vector of encephalitis. So people like Marston Bates, Dave Davis, Jorge Boshell, and later Pedro Galindo and Harold Trapido at the Gorgas Laboratory in Panama, all began doing extensive work in the forties, into the fifties, and even to this day on this completely different type of a cycle, which is the basic way in which yellow fever is maintained in nature in Africa and in the New World. So that's when they began to have to develop the larger and more diversified teams.

As a matter of fact, we knew what they were doing, but I think our studies in the early forties were going along very much in parallel. We talked in one of the earlier sessions about how yellow fever virus was first isolated from mosquitoes in Africa, and also Japanese B encephalitis virus in Japan, coincidental to our research in Yakima. So the development of field teams, I think, went along very much in parallel.

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<sup>1</sup>Edwin H. Lennette, Pioneer of Diagnostic Virology with the California Department of Public Health, Regional Oral History Office, The Bancroft Library, University of California, 1988.

The Rockefeller Foundation had to get vertebrate zoologists or, if they were not vertebrate zoologists, at least people who had very extensive training in this subject, whether they were physicians, entomologists, or whatever they were, to do work on the primates as an aspect of the problem. They had to have entomologists to work on the entomological aspects because when they got into doing the detailed studies necessary, the training in the other fields was not enough. We knew about what they were doing; they knew what we were doing. We weren't in competition with each other in any sense of the word.

Hughes: Did this new employment of entomologists have repercussions on curricula?

Reeves: Entomology taught in schools of public health and the medical schools usually wasn't taught by an entomologist but by a physician or parasitologist who was covering the whole array of diseases of importance in, say, tropical medicine.

Hughes: Was there a course in medical entomology in the department of entomology?

Reeves: There was a course in medical entomology that had been developed by Professor Herms when he came to Berkeley in the early 1900s. The content of those courses wasn't limited to medical entomology; it also included veterinary entomology. For instance, when Dr. Meyer first discovered western equine virus, most of the other diseases that were known at that time were virus diseases that were transmitted primarily in domestic animals. So there were only a half a dozen arthropod-borne diseases that were known. Yellow fever was one of them, and Rift Valley fever, Nairobi sheep disease, and other diseases that we talked about earlier. But basically, medical entomology dealt as much or more with arthropods as pests because of their attacks on man and the venoms that they inoculated and so on.

The other thing you have to realize is that parallel to the work that was going on in viruses, there was a lot of work going on in malaria, in trypanosomiasis, which caused sleeping sickness in Africa, and in Chagas disease in the New World. These are protozoan infections. Work also was developing very rapidly at this time in leishmaniasis, another protozoan disease. So there was a large array of diseases that were being discovered from 1900 on. Malaria and yellow fever were the first two, and then other diseases were added very rapidly.



### Medical Entomology

Hughes: Were you familiar with this literature as you began the Yakima study?

Reeves: Yes, completely.

Hughes: Is that because of your medical entomology?

Reeves: Yes, the medical entomology courses that we had at Berkeley. Herms at that stage had one of the first textbooks in medical entomology.<sup>1</sup> He was the head of the department of entomology at Berkeley and also a pioneer in the field of medical entomology, and he'd been very active in World War I as an officer in the U.S. Army. Then also he was the first person to establish mosquito control programs for malaria control here in the western United States. So there was a lot of very rapid development taking place.

Meanwhile, Dr. L. W. Hackett, who was in Europe with the Rockefeller Foundation, had written his book Malaria in Europe.<sup>2</sup> He had worked on malaria with the Italian and other European scientists. This book really expanded the work that had been done earlier by [Ronald] Ross in India and by the British in various places that led to a science of malariology. We were fortunate enough to have Dr. Hackett come to Berkeley in the mid 1940s when he retired from the Rockefeller Foundation. He moved here and had offices in the School of Public Health that I shared with him. He was the editor of the American Journal of Tropical Medicine after he retired here, and he assisted in our teaching. We had the unusual privilege of having people like that settle here. He came back, in part because he was born in Benicia, California. His father had been an army officer in the armory out at Benicia.

Later Dr. R. M. Taylor retired from the Rockefeller Foundation and came here. He was responsible for the first two

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<sup>1</sup>William B. Herms. Medical Entomology, with Special Reference to the Health and Well-being of Man and Animals. 3rd edition, New York: The MacMillan Company, 1939.

<sup>2</sup>L. W. Hackett. Malaria in Europe. London: Oxford Press, 1937.

editions of the arbovirus catalog,<sup>1</sup> but he also joined our faculty. So people such as Hackett and Taylor and Harald N. Johnson gathered together in places like this or others at Yale. The Rockefeller Foundation provided a building to house their research in the Department of Public Health at Yale University. The Yale Arbovirus Research Unit (YARU) was housed there and still is active there today. Many Rockefeller retirees did and still do use YARU as their base of activity. Examples would be Wilbur G. Downs and T. H. G. Aitken.

The field of arbovirology was developing in parallel in the academic programs here at Berkeley, at Yale, and elsewhere. Cornell had a strong program in medical entomology. R. Matheson wrote a textbook there on medical entomology, as Herms had done at Berkeley. So basically, medical entomology was a well-developed field in the 1930s. When you went to any of these schools, you learned about all of the arthropod-borne diseases; you didn't just worry about one disease.

Hughes: How much of an impediment, if any, did you find in not having an M.D. degree?

Reeves: It certainly didn't stop me from doing anything I wanted to do, working with insects. The medical people were not at all concerned about that. They were perfectly willing to have somebody like me do that work. I never found it an impediment, but some people in the same position found themselves being treated as technicians. I never was treated as a technician, so it didn't pose a problem to me. I was offered an opportunity to take an M.D. degree at San Francisco with a minimum of courses at the end of World War II but decided I didn't want to do it.

Hughes: Hammon treated you as an equal?

Reeves: As an equal.

Hughes: And yet he was older.

Reeves: He was senior to me by twelve years. He'd had extensive training in epidemiology. He'd had much more laboratory training than I had had in the newly developing field of virology because he'd worked with Zinsser and [John F.] Enders and others at Harvard.

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<sup>1</sup>R. M. Taylor, ed., International Catalogue of Arboviruses Including Certain Other Viruses of Vertebrates. Edition I, 1967, Public Health Service Publication No. 1760. Edition II 1975, Department of Health, Education and Welfare Publication No. (CDC) 75-8301.

### Laboratory Methodology in Virology

Reeves: The laboratory field in this whole area was just beginning to evolve. It was very simple to begin with, and it wasn't until after World War II that the methodologies began to expand very rapidly. We were stuck with the neutralization test and inoculation of mice, guinea pigs, and monkeys. The complement fixation test just came into use about the mid-1940s.

Hughes: Developed where and by whom?

Reeves: I can't recall where the basic test was developed, but it was being adopted very widely in virology and adapted to a variety of different viruses. The problem at that time was that research had to be done to differentiate which type of antibody was being detected and at what time period during the course of the illness. Studies were done in order to understand what value these alternative tests had diagnostically. That was not the area that I was particularly concerned with except to have to know enough about the test to use it as a tool. So I wasn't really involved or concerned with the development of the methodologies as much as with their utilization once they were developed.

Hughes: But people in the Hooper Foundation presumably were?

Reeves: Not with the theoretical aspects of the test. The conceptual aspect of a complement fixation test or a neutralization test or a hemagglutination inhibition test really was not a focus of interest at the Hooper Foundation. The Hooper Foundation's function was to apply these tests to understand how diseases were transmitted and where they were occurring and so on. So it was much more focused on epidemiology than it was on methodology. Hammon very early on published on the adaptation of these tests to field studies.<sup>1</sup>

The other thing that you have to realize when you bring up Dr. Meyer's name was that in the same time period he was rapidly developing the field of plague. There was a need to have a

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<sup>1</sup>W. McD. Hammon, E. M. Izumi. A virus neutralization test, subject to standardization: used with western equine encephalomyelitis, St. Louis encephalitis, and mouse-adapted poliomyelitis virus. *Journal of Immunology* 1942; 43: 149-157.

W. McD. Hammon, C. Espana. A simple method of producing control guinea pig immune sera for use with complement fixing antigens for the arthropod-borne virus encephalitides. *Proceedings Society Experimental Biology and Medicine*, 1947; 66: 113-115.



detailed knowledge of the flea vectors and the animal reservoirs of plague. So he was organizing teams of workers and showing leadership in getting state health departments and the Public Health Service to recognize the importance of having a very broad array of sciences being brought to bear on a disease like plague.

### Research on Other Arthropod-borne Diseases

Hughes: In Meyer's foreword of 1962 to your book on the encephalitides, he says, and this is paraphrasing, that the interdisciplinary team assembled to study encephalitis in Yakima was based on the experience gained in studies of sylvatic plague in California.<sup>1</sup>

Reeves: Well, yes, as I said earlier, in the thirties he was a leader in recognizing the importance of bringing other sciences to bear on these problems, not just studying them as a disease phenomenon in mankind. He was one of the fathers of the whole field of zoonotic diseases. Once you recognized in studies of zoonoses that many were infections of wildlife, with or without an arthropod vector of some type, it made no difference whether it was relapsing fever, which was a tick-borne disease that was in the high mountains in the Tahoe area; relapsing fever in the epidemic form, which was occurring in Europe and had a body louse as a vector; or plague, even Q fever, which Dr. Lennette worked on here in California.<sup>2</sup> It not only could have a direct cycle from the animal host to man or from animal to animal, but also at times ticks or other arthropods became involved as vectors.

Actually, the first evidence of a tick cycle for Q fever was found by [E. H.] Derrick in northern Australia around Brisbane in his studies with Burnet. The agent was isolated almost at the same time from *Dermacentor andersoni* ticks by H. R. Cox and C. B. Philip at the Rocky Mountain Laboratory. They didn't know what they had, and they actually found out the hard way. Dr. Rolla Dyar, one of the top people in the National Institutes of Health, came to visit them at the Rocky Mountain Laboratory, and they proudly took him around showing him their various discoveries and took him into the animal room where they kept guinea pigs infected with this agent which they'd isolated from ticks. He was considerate enough to go back to Bethesda and get very ill and had

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<sup>1</sup>W. C. Reeves, W. McD. Hammon. "Epidemiology of the Arthropod-borne Viral Encephalitides in Kern County, California, 1943-1952." University of California Publications in Public Health, Vol. 4. University of California Press, Berkeley, 1962.

<sup>2</sup>See the oral history with Dr. Lennette.

to be hospitalized. He happened to be reading the Australian Medical Journal, and he got on the phone. He called them and said, "I think I know what I have, and I think I know what your new agent is that you are calling Nine Mile Fever. Read the Australian Medical Journal and the article by Derrick and Burnet. I think you'll find that it is Q fever."

You had to get out in the field and study where these things were coming from and how they were operating in their basic cycles in their animal hosts, not only in sick people. Man's infection was an accident. Man also was an accidental host in plague infection unless he got pneumonic plague, which can spread from person to person. But the plague we have here in the western United States is a wildlife disease in small rodents and in the flea vector. When man or his pets, like dogs or cats, get involved, it's purely an accident. They're not essential to the survival of the organism.

Dr. Meyer brought onto his staff people like Dr. Charles Wheeler, better known as Buzz Wheeler; Barbara McIver; Bob Holdenreid; James Douglas; and others. He had a whole array of these people who were entomologists and who made major contributions in the studies of plague, relapsing fever, and other diseases. You have to realize that the professional society here in the Bay Area was a very close one at that time, with Dr. Herms, who was head of entomology, and Dr. Meyer at the Hooper Foundation. These people saw a lot of each other and exchanged ideas, exchanged problems, exchanged people, so that while it wasn't a religion, it was a science that almost was like a religion in regard to what was necessary to do a successful study.

### Chasing Epidemics

Hughes: Meyer goes on to say that your team had the opportunity to study western and St. Louis in Oregon, Texas, Nebraska, New Mexico, Arizona, Oklahoma.<sup>1</sup>

Reeves: That's true.

Hughes: Do you want to go into any of that?

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<sup>1</sup>Meyer's forward in Reeves, Hammon, Epidemiology of the Arthropod-borne Encephalitides, pp. iii-iv.

Reeves: I think there are a couple of interesting sidelights. We talked a little bit earlier about how we were called into these various areas, sometimes by the army, sometimes by the U.S. Department of Agriculture, sometimes by a state health department. Usually it was a response to an epidemic. At that time, the concept was that if you really wanted to learn about these diseases, you had to get in on an epidemic to find out how it was spread and what was going on.

In the Yakima Valley area we were indeed successful in this regard, because we were able to get in on epidemics when they were quite active and find out what was happening at the time of the epidemic. We also were successful in Kern County. Let me give you some contrasts.

#### More on the 1941 Encephalitis Epidemic in Texas

Reeves: As I told you, in 1941 there was an epidemic in the lower Rio Grande Valley of Texas around Brownsville and San Benito. Hammon went down there to do a quick investigation, as he had done in Yakima in 1940, and he found out that eastern, western, and St. Louis encephalitis had all been active at the same time in the same epidemic. He had clinically proven cases of all three, and this was a unique situation. But he got in at the tail end of the epidemic; there wasn't any opportunity to do an investigation of what the vectors were or how this epidemic was spread. So in 1942 the Texas State Health Department invited us to come there to work with them, anticipating, frankly, that there would be another epidemic.

So I went down there, and another graduate student from the entomology department, Bernard Brookman, went along. We went down and were joined by Richard Eads of the Texas State Health Department to do a big study of the vectors and how these viruses were being spread, because this was a unique thing--all three viruses at one place.<sup>1</sup> We'd had two viruses in Yakima, but now we had three. And eastern encephalitis virus wasn't even known to be occurring that far west in the United States at that time.

Well, we got down there, and there was a horrendous drought. There was no rain. There was very little water standing around.

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<sup>1</sup>W. McD. Hammon, W. C. Reeves, J. V. Irons. Survey of the arthropod-borne virus encephalitides in Texas with particular reference to the lower Rio Grande Valley in 1942. Texas Rep. Biol. Med. 1944; 2:366-375.



The water that was in the resacas (a permanent body of water in an old ox-bow of a river) and in water pipes was so salty that you couldn't drink it. You went over to drinking grapefruit juice and beer. And we could hardly find any mosquitoes. We were out doing the best we could and freezing them up for virus tests, but we never got any virus out of them. There was no clinical infection whatsoever. We went out and got blood samples from animals. We'd find hardly any antibodies in any of them unless they were older animals from the year before.

What we learned was that you cannot anticipate these epidemics. If you have a drought, you may not have enough mosquitoes for anything to happen. So the best we salvaged out of that was negative information. At the same time, I did some of the very first laboratory studies in which we showed we could infect *Culex tarsalis* with St. Louis virus, for instance, and some other *Culex*<sup>1</sup>. But basically I couldn't even get enough *Culex tarsalis* in the Rio Grande Valley to do the studies. I had to go all the way up to San Antonio to make the collections and bring them back down to San Benito. I drove a couple of hundred miles each way in a car with no air conditioning. But we managed to do the experiments under very primitive conditions. The labs we have now are real castles compared to what we had then.

Hughes: What was the lab?

Reeves: We moved into the San Benito Health Department. It was a very small county health department--a health officer, a couple of nurses, and a sanitarian that was on loan from the Public Health Service. They gave us an office about the size of this one--say, fifteen by twenty--and that was our laboratory. I couldn't do any mosquito experiments or work with animals there, so I said, "You must have some more space around here, as I have to have chickens, rabbits, guinea pigs, mosquito cages, and everything." The health officer said, "Look, there's no more space here." So I started nosing around in this old adobe type of building and ended up on the roof. There was a dance floor up there for community dances. This was in San Benito, and it was a very small city; still is. The Stonewall Jackson Hotel is the only place to stay there until you get a house to live in. On the roof they had a place, again about fifteen by twenty feet, where they had dispensed soft drinks at dances. The pigeons had moved into this place. There was no screening, and the doors were just loosely hinged. It was just an adobe room with nothing in it except dirt, pigeons, and plywood.

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<sup>1</sup>W. McD. Hammon and W. C. Reeves. *Culex tarsalis* Coquillett, a proven vector of St. Louis encephalitis. Exp. Biol. and Med. 1942; 51:142-143.

I said, "How about this space?" He said, "If you can use it, it's yours."

So I got a hose. I washed it all out, put in good doors and screens, painted it, and so on. Built screen partitions across it. I didn't have to worry about keeping my mosquitoes warm. I also didn't have to worry about keeping warm, because it was 90 to 100 degrees up there every day, and the humidity was intolerable. I set up mosquitoes and colonized some of them, brought them in from the field. I got Señor Carlos Chavez, a local Chicano gentleman who was in his sixties who also was the interpreter for the local judge when cases came in who didn't speak English. Charlie was my interpreter. He was my animal man and passport onto many properties. He also worked in the hardware store, so he knew everybody. We got along beautifully and worked day and night.

So we were able to do some experimental work. But talk about primitive, I mean, it was really primitive. I'd take about three or four T-shirts with me every day up there. I'd wear them for an hour or two, and then I'd hang them out in the sun to dry and put on a dry one. I took salt tablets every day. It was primitive. We were there for almost three months, and had a great time. Became members of the local society.

Hughes: What about the studies in other states?

#### More on the 1944 Encephalitis Epidemic in Oklahoma

Reeves: We went to Oklahoma in 1944, which I think I mentioned briefly in an earlier session, to investigate the encephalitis epidemic there. Again, we got there too late, so there were no cases happening. It was all over. They'd had the first frost; there were hardly any mosquitoes around.

Hughes: The military called you in?

Reeves: It was the military calling because they had those fancy [Lippizaner] horses from Austria.

The only thing that I learned that I didn't know before I went there was one day when I was out chasing down a supposed horse case. It was very difficult to find these cases. The veterinarian would say, "There's a horse case," and you'd go so many miles that way and so many miles someplace, and you'd drive up this creek bottom, and you'd find Mr. Jones or whatever his

name was. Our only transportation was a military jeep that had been assigned by the army, with a Major Harry Rubin who was in full uniform, and the jeep had a star on the side. Well, everybody thought we were the revenue agents out looking for their still.

One day I pulled up in front of this place, and there was an old man sitting on the porch of this house. I went up and carefully explained to him what I wanted to do and what I was interested in. He said, "Sonny, you don't have any further to go. I'll tell you where that horse disease comes from. It's that yeller dust." I said, "That's interesting. What's yeller dust?" He said, "You see that plant out there with all them yellow flowers on it?" It was goldenrod. He said, "When all that yeller dust comes off of that, the horses get that disease." I said, "Okay. By golly, I didn't know that before I got here. Now that I know that, I can relax, but would you mind if I look around for some mosquitoes?" He said, "If you can find mosquitoes, they're all yours." [laughter] So I went ahead and collected the mosquitoes, and he was happy and I was happy. I put yeller dust in that interesting category, back in that little pigeonhole over here, but I didn't think I was going to pursue it as a source of viruses. Anyway, we got little out of that except the yeller dust.

#### The 1948-1949 Japanese B Encephalitis Epidemic on Guam

Reeves: We went to Guam in 1948-49, which is a little bit ahead of this time period, but there was a Japanese B encephalitis epidemic there in 1947. Hammon had gotten in early enough in the epidemic to diagnose it, but no detailed epidemiological studies were done. We went out there and spent three to four months of the best parts of Al Rudnick's and my life chasing mosquitoes, but there was no more virus there. So we described the mosquitoes of Guam,<sup>1</sup> described the first occurrence of an *Anopheles* mosquito there, and showed that the *Anopheles* mosquitoes had been introduced from the Philippines. They're still there. Later they had malaria transmission on the island.

Hughes: Introduced by whom?

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<sup>1</sup>W. C. Reeves, A. Rudnick. A survey of the mosquitoes of Guam in two periods in 1948 and 1949 and its epidemiological implications. American Journal of Tropical Medicine and Hygiene 1951; 31: 633-658.



Reeves: Probably by the U.S. Marine Corps that was doing amphibious maneuvers between the Philippines and Guam. In addition, Filipino labor was brought to the island, and they came from highly endemic areas of malaria in the Philippines. Dr. William Reisen, who is now head of our field laboratory in Bakersfield, followed me some ten to fifteen years later in Guam and again described the mosquitoes of Guam. He was in the armed forces then. Now we're working together in Kern County.

All we could do in the late 1940s was reconstruct as best we could what the probable vector was. Guam had a mosquito, *Culex annulirostris marianae*, and it looked and acted a lot like *Culex tarsalis*, and indeed, it's turned out that that was probably the vector there.

I went to Australia in '51-52, chasing a Murray Valley encephalitis epidemic. In 1951 they had a big epidemic in the Murray Valley. When we got there, there were no more cases. There was no more virus activity, so again all we could do was reconstruct what the probable vector was. Again, we thought it was *Culex annulirostris*, and it turned out later, when they did have epidemics, that was the right vector.

We developed the concept that the cheapest control program you could buy for encephalitis was to invite us to study it, get us to go to the place, and it would disappear. This was an epidemiological lesson. With these particular infections, which are infections in wildlife and depend on a cycle in nature and upon having an excess of vectors and rainfall and so on, you can't guess when they are going to happen next.

There's an old saying, "If I had God as a consultant, I could do much better." But I don't happen to have as reliable a consultant. You take your chances. Sometimes you go and you're successful--Yakima Valley, Kern County, and our current studies in southern California in the Imperial and Coachella valleys. We've found areas that are consistently what we call hot areas, where the virus is there and active. We'll be talking about some of those later.

Chasing epidemics is a risky business. They had a big epidemic in the early fifties in the Tampa Bay area, and the Florida State Health Department set up a big project to study it, and they didn't get any more cases until ten years later. So basically this is the lesson of chasing epidemics. Florida had its next epidemic in 1990.

But chasing epidemics or periods when there is no or little active infection still can be profitable. You can learn about the

insects that are there, and you can study the epidemic in retrospect. You can get blood samples from people and see what proportion of people have been infected earlier. You can do blood samples on animals of different age groups and determine what proportion of them were infected earlier, even though there's nothing going on currently.

Hughes: The military was satisfied with what you came back with?

Reeves: Yes. What else could they do? They had to be satisfied; somebody went out and did his best. Plus the fact that the information we were getting overall was providing them a firm basis upon which to evolve control programs and a knowledge of a wide variety of viruses.

### The Encephalitis Field Study, Kern County, California, 1943

#### Reasons for and Goals of the Study

Hughes: I want to get into control, but let's first home in on Kern County. Why was Kern County chosen?

Reeves: Okay, we chose Kern County for work in the 1943 period because we'd done the work in the Yakima Valley in '41-42. We'd found out what the primary vector was there and how it could transmit virus and how animals were the source of infection, not man. Now, we looked for another field area where we might confirm what we'd found in the Yakima Valley. We knew about Kern County because Miss Howitt at the Hooper Foundation had been studying that area, and she chose it because it had such high rates of central nervous system diseases. She was interested in poliomyelitis, she was interested in encephalitis, and she did some excellent work on those diseases in the late thirties. Actually, she started in the mid-thirties and worked in Kern up until 1940.

As I said earlier, I was due to go down to work in Kern County in 1941 with Miss Howitt when the Yakima Valley project developed, and Dr. Meyer made the decision that it would be best if I went with Hammon to Yakima instead, because Miss Howitt really did not work in the field; she worked in collaboration with the health officer and assistant health officer, Dr. William Buss and Dr. Myrne Gifford in Kern County. They provided all the clinical information and samples to her, but she never really went down there and did any work herself.

Hughes: She stayed in San Francisco?

Reeves: She was a laboratory person. She stayed in San Francisco. She might go down to visit, to talk to them or something, but she didn't even go in the hospital there and do any work.

So Hammon decided that we wanted to pursue the findings we had made in the Yakima Valley and determine if the same would be found in Kern County.

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Reeves: We knew western and St. Louis virus were both in Kern County because of Miss Howitt's work. Kern County was much closer to San Francisco than the Yakima Valley, so now we only had a three-hundred-mile trip to make instead of an almost thousand-mile trip. We wanted to see what the vectors were in Kern County as we'd done in Yakima, not assuming that it was going to be *Culex tarsalis*, looking at everything that was there as much as we could, looking at *Aedes*, looking at the other ectoparasites, animals, and so on.

We wanted to do the study the best we could because of the National Foundation's interest in unscrambling the enigma of the different viruses that were causing central nervous system disease--polio, western equine, St. Louis, and so on. The polio foundation was actually the principal, almost the sole, supporter of our project at that time. So we were just as interested in polio as we were in western equine and St. Louis encephalitis. That's why I did some work down there on flies and polio, which was not at all productive.

So we went down there with just one objective: are mosquitoes infected with viruses? If they are, then we can go to work on their efficiency as vectors and so on. Two of us went down initially, Pedro Galindo and I. Pedro and I still correspond, and he still lives in Panama. He later became the director of the Gorgas Lab there and did fantastically good work on yellow fever and encephalitis viruses in Panama.

#### Isolating Western Equine Encephalomyelitis Virus

Reeves: We went down, and the first night we set out a couple of our mosquito traps at the Kern Land & Cattle Company holdings in the suburbs of Bakersfield. We made a big collection of mosquitoes that night, and we got two western equine virus isolations out of the first collection. So we isolated western virus in the very first night's collection, and it came out of guess what species?



Hughes: *Culex tarsalis*.

Reeves: *Culex tarsalis*. Well, that was the beginning of a very happy summer, because Pedro is an excellent field entomologist, and the two of us just went out there and beat the area to death. I mean, we collected mosquitoes like crazy. We collected them resting in shelters, we collected them in light traps, we collected them off horses. We not only isolated western and St. Louis virus both out of *Culex tarsalis*, we also found California encephalitis virus in *Aedes* mosquitoes, and that was a new virus at that time. It later was designated the original member of the California sera group, which has become important elsewhere, not in California particularly but with the closely related LaCrosse and Jamestown Canyon viruses that cause disease in the Midwest and the East Coast.

The principal thing we did that year was to find that of all the mosquito species, and there are some twenty-two species in Kern County, only *Culex tarsalis* was consistently infected with western equine and St. Louis viruses.

Hughes: But you did find occasional infections in other species?

Reeves: Occasionally in others. This set up the scene so that we knew what we had to do in the way of confirmatory tests, as we'd done in Yakima. The questions were: can *Culex tarsalis* from Kern County be infected and transmit these viruses? When we found other species infected with these viruses, then the question was: were they just accidentally involved because they'd happened to feed on an animal that *Culex tarsalis* had infected, or were they important vectors? And indeed, the former usually proved to be the case.

The exception was California virus that we got from *Aedes melanimon* mosquitoes, and it turned out that its cycle had nothing to do with birds at all, because the *Aedes* mosquitoes we found infected didn't feed on birds. They fed on large mammals, from cottontail rabbit size up to cattle, horses, and so on. That virus turned out to be an infection of mammals transmitted by *Aedes* mosquitoes, which is a counterpart of the *Culex tarsalis* cycle. Some years later we found out that *Culex tarsalis* also frequently feeds on jack rabbits and can maintain a separate cycle of western equine virus; and the *Aedes* mosquitoes that carry California virus also can pick up western virus from the jack rabbits and then can transmit it to other hosts such as man and horses.

## Mosquito Control

Reeves: One of the other reasons that we selected Kern County was that this was one of the first areas in California to develop a mosquito control district. It was named the Dr. Morris Mosquito Abatement District, which later was renamed the Kern Mosquito Abatement District in 1945 and is now designated as the Kern Mosquito and Vector Control District. They developed mosquito control in Kern County in the early 1900s because malaria was so important that they really couldn't develop the city and the agricultural industry there until the disease was controlled. Malaria was such an important problem, as it was in other parts of California--in the foothills and in northern California. They had to get the Kern River leveed so that it wasn't creating one great big marsh, which they did. Once they'd done a good job on malaria control, the city went ahead and developed, and agriculture developed. Malaria disappeared.

Here again, we came in and showed that encephalitis was carried by mosquitoes. Kern County had the highest rates of polio and encephalitis of any county in the United States, and that's another reason why we selected it for study. Now we had another thing for the mosquito abatement district to do. We had identified a mosquito as a vector that nobody was trying to control; they didn't think it was of any importance. An engineer named Fred Hayes was in charge of the mosquito control program. He was willing to expand their program in 1946 regarding what species they were trying to control, once we showed which one was important.

We were chagrined, because one of the objectives of the Yakima project had been to gather enough information to control the encephalitis epidemics up there. Mr. Gjullin had been there with us working on the control aspect. He had submitted a report to the Yakima County Health Department about how they might establish a control program. That went along with our reports about the importance of *Culex tarsalis* as the vector, but by the end of 1943 it was obvious that Yakima hadn't done any control. The reason was that the disease had disappeared. There just weren't any more people getting encephalitis in that area. They said, "The problem's gone away. Why worry about it anymore? Thanks a lot."

So in 1944 I got curious about this. In August that year, I said, "I've got to go up to Yakima again. I have to see what's going on up there." So I loaded Bernardo Granadino from San Salvador, who was the only person I had to work with me that summer, into my car. He was a pre-med student here at Berkeley



and wanted a job for the summer. So he followed me around the whole summer at two paces behind.

Bernardo and I took off, and he didn't drive a car; I found that out the hard way. I let him drive for five miles on a mountain road one day, and that cured me. [laughs] Anyway, we went up to Yakima, and the mosquitoes were still infected with virus; the birds were still being infected at rates that were even higher than they'd been in some of the previous years. What we found out was that a very high proportion of people up there had been immunized by bites of infected mosquitoes, because most people who get infected don't get the disease. So for practical purposes mosquitoes had been flying around vaccinating people with virus.

So then we forgot about Yakima; they weren't going to do anything about mosquito control if there wasn't more disease. But in Kern County we'd go ahead in developing control programs. The additional information on this disease, which was important to that community, allowed the Kern Mosquito Abatement District to expand the size of its district from some eighty square miles, which was just the immediate city of Bakersfield, to some 764 square miles, within a couple of years of our discoveries. That got rid of some of our best study areas, because they'd go in and do control, and then it no longer was a good study area. We had to go out further, and we kept going out further, but finally the district got to be over 1,500 square miles, almost to the edge of the valley portion of Kern County, so we concentrated our studies on how they could do a better job of control.

Hughes: Were your findings shaping their control program?

Reeves: In large part, but they still had other problems. They didn't want malaria to come back, but that seemed to be a minimal problem. Malaria for practical purposes had been eradicated from California, but what was happening meanwhile in the rest of the world? Well, we had huge populations of soldiers overseas in the 1940s getting infected with malaria. They were all going to come home. At that time we had no cure for malaria. We had treatment for malaria to minimize the disease, but no real cure was available. So the approach was to suppress the disease with atabrine, but the infection was still there. There was a great deal of excitement in the state legislature and all the Central Valley of California about what was going to happen, because the war was coming to an end in 1945. What's going to happen when all these malaria-infected people come back home? It was assumed we were going to have epidemic malaria again. The state legislature was very concerned about this, and they were in the process of



passing legislation to provide extra money to prevent this happening.

That's when I developed the slide of the poster that I showed you, of our concept of the transmission cycle for western virus. Those of us who were in the encephalitis business weren't against malaria eradication and control, but we also thought that encephalitis deserved some attention. So we were invited to make presentations to the state legislature about encephalitis and urged it to include encephalitis in its financing of mosquito research and mosquito control in the state. The end product of that was that they allocated \$600,000 in 1945-46 for extended research on encephalitis and extended control of encephalitis and malaria. Now, \$600,000 in today's world doesn't sound like much, but in 1945-46 that was a lot of money. And that was the first support we had from the state legislature for research in this area and money to improve and expand the local control programs.

Hughes: Were you directly involved in lobbying?

Reeves: I had that poster made, and I presented it to the state legislature. It shows a central cycle of western equine encephalomyelitis infection between birds and mosquitoes and that this is maintaining the virus, but then these mosquitoes will diverge off and feed on other blood sources, and when they feed on people and horses, they infect them.

It was primarily a disease of infants and children and young people, but occasionally was in adults. When that happened, it could be a very serious disease, including death and long-term residual effects. But the infection went no further. The infection wasn't carried from person to person like measles, chicken pox, or polio. People didn't have enough virus in their blood to infect mosquitoes. At the same time, the same mosquitoes might feed on horses, and if they did, the horses were just like people; they could get infected with western virus and get sick and die. This was very important in an agricultural economy that was still dependent in large part on horsepower. That again was a dead end. The mosquitoes didn't pick the virus up from horses and carry it further.

When I presented the poster that presented those concepts, I was very, very fortunate, because one of the senators said, "That's my horse. My horse died of encephalitis last year." I thought, "Boy, how lucky can I be? I've got a man on the legislative committee who understands the problem." And I couldn't believe it when another legislator said, "And that's my grandchild, with the residual effect." The odds against having two people on that committee who had had that experience in their

families was very, very small. But it also shows you how common those diseases were then. People in the rural communities knew about them.

In 1945 the war was coming to an end. Actually, when we started that summer, we knew the war was coming to an end, because it basically had in Europe, and the Pacific war was winding down very rapidly.

### **Spraying with DDT**

Reeves: Meanwhile, the U.S. Government had gotten very involved in the new insecticide, DDT. This was something brand-new. It was developed by the Geigy Company in Switzerland. DDT was originally just a laboratory exercise by a chemist in Switzerland to see what he would come out with if he put these chlorinated hydrocarbons together in certain configurations. He had no objective in doing this except as a scientific chemical experiment. So he developed this new material, and he didn't know what it was good for. The Geigy Company was making this stuff and looking for uses.

It was said that our military intelligence discovered that the Germans bought some railroad cars full of this stuff to be brought to Germany. So they got very excited about it, because any time the Germans bought something like this, we'd better have some, too. When that material got put into the intelligence pipeline here in the United States, part of it got sent to Orlando, Florida, where the U.S. Department of Agriculture was screening any chemical they could get their hands on to see if it would be of any value to control insects. They had thousands of products. When this product hit that lab--and this was a classified research area, top secret--it killed any insect that got within five miles of it. I mean, it was really something. It killed lice, it killed fleas, it killed mosquitoes, it killed flies--anything you wanted. For anything that was an arthropod, it was fatal.

By 1945, the armed forces had started shipping it out to the Pacific theatre, and they did extensive mosquito control programs by airplane in areas like Okinawa before it was even invaded. They went in and sprayed Okinawa from top to bottom, and other places--the Philippines and so on. But meanwhile DDT wasn't available to civilians as far as use was concerned, although we were beginning to hear something about it.



In 1945 we were able to get several hundred pounds of pure DDT shipped to us in Kern County to carry out an evaluation of its effectiveness for encephalitis control. We did this through my contacts with the Malaria Control in War Areas Program, which was a part of the United States Public Health Service. That program later evolved into the Centers for Disease Control of the Public Health Service that's now centered in Atlanta, Georgia. The Malaria Control in War Areas Program was to prevent malaria from being reintroduced into the United States. So they had DDT, and I also had my contacts with the army through our work that we'd done in various places for them.

Hughes: Who arranged for the DDT shipment to the Hooper Foundation?

Reeves: We did at the Hooper Foundation--Dr. Meyer, Dr. Hammon, and me, with our contacts we had in these places. They finally declassified DDT. They just sent it to us, a couple hundred pounds of pure DDT in a barrel. It arrived on a train. We'd developed a protocol to see if we could really control and eradicate encephalitis by using DDT. As far as I know, this was their first release of DDT for this sort of research.

In 1945 we went out and sprayed eighteen square miles of Kern County with DDT. In ten and a half square miles we did every chicken house. Our concept was that chickens, which were in everybody's backyard at that time because of the war and meat rationing and egg rationing, were all being infected with encephalitis virus. If we sprayed all the chicken houses, and *Culex tarsalis* was sitting inside the chicken houses and feeding on chickens, we might control the disease. Then we did another ten and half square miles of spraying, and that included 120-some chicken houses and other shelters where mosquitoes were resting. Almost every ranch had chickens.

Hughes: How many of you were doing the spraying?

Reeves: I had G. Edwin Washburn, who was a commissioned officer in the Malaria Control in War Areas Program and who wore a natty uniform every day, but he went out and did spraying in his uniform. If he didn't, I might have killed him. [laughs] We had an engineer named Oscar C. Blumberg from the state health department, who thought he was coming down to direct a project, so he came down wearing a suit and a necktie, but he also had to go out and spray in his suit and necktie. I had C. Eugene Snyder, a sanitarian from the state health department who was assigned to the project. I had two plague control people that were workers who usually went out with shotguns and shot ground squirrels and autopsied them and collected fleas for plague research. So six of us got our little hand sprayers out and we got some good old light diesel oil and



mixed up 5 percent DDT in the diesel oil, which is what the people in Orlando, Florida, told me to do. We went out and sprayed chicken houses.

Hughes: With no fear of the toxicity?

Reeves: No. What the hell's there to be afraid of? There was a barrel of that stuff sitting out there. You got a scoop and reached in and got this stuff out and weighed it, put enough in a big barrel of diesel oil to dissolve it, and mixed it all up with an outboard motor. We did this at the mosquito abatement district, because I didn't want to kill all the mosquitoes in my laboratory at the Kern General Hospital. Everyone stood around and watched me do it.

Hughes: It didn't ever run through your mind that if it was fatal to mosquitoes it might not be so good for you?

Reeves: It hadn't killed anybody in Orlando, Florida, where they were working with it, no problems had been reported by the army, and they'd done all sorts of animal toxicity tests and had no problems. Rachel Carson hadn't woke up yet. We had DDT all over the place.

One time our engineer showed up again dressed in his suit and necktie, and this plague control guy took a dim view of this because he wanted him to work. So he was pumping up this spray can, and it just happened that he had an accident and this thing turned on the engineer full force. The guy kept pumping it and spraying this guy from top to bottom. The engineer got sort of mad but he changed his clothes and went to work. We had the stuff on us from top to bottom. We were covered with it every day we went out and worked.

Hughes: Did you have any trouble with the owners of the chicken huts, wondering why this oddball group was coming to spray?

Reeves: No, we told them, "We're coming out here to try and control this disease," and they knew what that disease was. A lot of the people in these neighborhoods had already lost horses, or they had kids who had been infected with the disease. The newspapers in Kern County were full of headlines about encephalitis and polio, and we were saviors. It never occurred to us [to worry about DDT]. As a matter of fact, I'll be very candid about it right now. You could spray me from head to foot with DDT today, and I probably wouldn't flinch, okay? I wouldn't flinch, and it's not because I'm seventy-four years old. Some people will say I'm stupid.

Now, that doesn't make me popular, I grant that. There are people who take great exception to this. In our field experience we had no indication whatsoever that DDT had an adverse effect on chickens or on pets or on the people in these areas.

Hughes: How successful was this spraying as a control measure?

Reeves: Didn't work at all. [laughter] Actually, the first failure we had was a little embarrassing, because I'd taken the advice of the people at Orlando, Florida, who said, "Put it in light diesel oil, 5 percent suspension, spray it on the surface evenly so you get 200 milligrams per square foot or whatever, and you'll be able to see the crystals on the wood if it's done correctly, and that's it."

So we went out, and, you know, those people in Florida had never seen a Kern County chicken house that had been sitting out there in the desert for maybe twenty years. When you sprayed the diesel oil on the wood, it just went in like that, and phttt!--it was gone. It just went right into the wood. We were trying to get residuals off of the surface, and they had a toxicologist down at the Riverside experiment station, F. A. Gunther, who was doing evaluations of how much was on the surface of this wood. He kept saying, "You guys must not be putting it out. I can't find any DDT."

So I had to dig around and figure out what we were doing wrong. It was obvious that dry wood was just soaking up all the stuff and taking it in. So I communicated with all the experts I could find, which were none, and finally somebody said, "You have to make a quick-breaking emulsion in water, and as soon as it hits the surface the emulsion will break and the DDT will be on the surface." I won't go into more detail, but Triton X-100 was the emulsifier I had to use. At first I thought it was motor oil, but that wasn't it. So I used xylene, water, and an emulsifier to make a 5 percent solution.

Anyway, so I had to go back to Mr. Chester Gillespie, who was the engineer in the California State Health Department who was handling the state funding for the project. I said, "Mr. Gillespie, we're going to have to spray again. What we did was wrong." He said, "What do you mean it was wrong?" I said, "It didn't work." I explained to him why, and he said, "Well, if that was wrong, why should I believe you're going to do it right the next time?" I said, "Mr. Gillespie, we're out in no-man's land, and we're going to have to try what the experts have told us to try as the next alternative. I hope it works." "Well," he says, "I'll give you money one more time." I said, "Mr. Gillespie, this isn't your money. The state legislature gave us this money to do

this work." He said, "But I'm responsible." He was an old, hard-nosed engineer, and he never came to Kern County to see the project. So he gave me the money, and we did it the next time and got good crystals on the surface. We knocked all the mosquitoes flat on their backs that were in the chicken houses or any other place that we sprayed.

Hughes: You hadn't before?

Reeves: No, not before. But having done this, we had absolutely no or very little effect on the amount of virus that was in mosquitoes in the area or upon the transmission rates. The reason was that we had completely misinterpreted the importance of chickens in chicken houses. It turned out that we did not reduce the overall mosquito population, because most of them never went into the chicken houses. Most of them sat outside and fed on wild birds. It was very fortunate we did this experiment, because it led us completely away from the chicken idea and made us realize how important wild birds were. We published the study in two papers.<sup>1</sup>

### Surveying Wild Birds

Hughes: You immediately more or less dropped the chicken idea and went to wild birds?

Reeves: Yes, and we did extensive surveys on wild birds. We employed an ornithologist, Dr. H. Elliott McClure, who was getting out of the navy at Vallejo. He was an ornithologist from Nebraska who had done his master's thesis on pheasants and his doctoral thesis on mourning doves. So we had an instant bird expert who was glad to get out of being an entomologist in the navy. He was a very, very dedicated field man, worked day and night. He wrote a paper

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<sup>1</sup>W. C. Reeves, G. E. Washburn, W. McD. Hammon. Western equine encephalitis control studies in Kern County, California, 1945. I. The effectiveness of residual DDT deposits on adult *Culex* mosquito populations, American Journal of Hygiene 1948; 47: 82-92.

W. McD. Hammon, W. C. Reeves. Western equine encephalitis control studies in Kern County, California, 1945. II. An evaluation of the effectiveness of certain types of mosquito control including residual DDT on virus infection rates in *Culex* mosquitoes and in chickens, American Journal of Hygiene 1948; 47: 93-102.



called, "Ten Years and 10,000 Birds."<sup>1</sup> So we went wholesale on the wild birds, and it really got us into the business of how important they were--the house finches, house sparrows, doves, blackbirds, and so on.

Hughes: Why were you convinced at that stage that wild birds were important reservoirs of the virus?

Reeves: We felt we had probably eliminated any probability that chickens were an essential source of virus, plus the fact that now we realized we had to learn more about this mosquito's biology and what it really fed on as far as blood preferences were concerned. We didn't have the precipitin test, which we developed later, so we really couldn't identify bird species. All we could do was to say that the blood in mosquitoes had come from a bird. We had a test that was very sensitive to determine that much, but we didn't have a test that would take it down to individual species.

We cast about to find some way to find this out. Fortunately, I had a friend who was working for the California State Fish and Game Commission at that time, Carlton M. Herman, who had a Ph.D. in parasitology from Johns Hopkins. He was a personal friend of mine, and I don't remember how or where we'd met. He was one of the world's top experts on bird malaria, and birds are infected with many different species of malaria. Their malarias are not transmitted by *Anopheles* mosquitoes like human malarias, but by *Culex* mosquitoes. So we decided to do a study on bird malaria, the first big field study that had ever been done on bird malaria. Our objective was to see how many species of malaria were infecting which birds and *Culex tarsalis* and the other mosquitoes that were feeding upon the birds.

To make a very long story short, we did thousands of blood smears on birds, we did thousands of mosquito dissections, all to find out what the malaria parasite rates were. We found there were seven species of bird malaria in the area. We found that *Culex tarsalis* was the primary vector of bird malaria, and two other mosquitoes, *Culex stigmatosoma* and *Culex quinquefasciatus*, contributed some to transmission. We found that many different species of birds were being infected with malaria even as nestlings, before they left the nest. So by doing a definitive study on bird malaria, which led to three papers in this time

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<sup>1</sup>H. E. McClure. Ten years and 10,000 birds, Journal of Ornithological Investigation 1962; 33: 1-21.

period,<sup>1</sup> we actually were able to confirm that this mosquito preferred to feed on wild birds and fed on them commonly. That really set us in motion and then led us to developing the right reagents so that we could identify with different tests what the mosquitoes were feeding on.

### Methodology

Hughes: Do you want to talk next about the development of the methodology, particularly the tests?

Reeves: There are two methodologies that we developed in this discovery decade as a result of these particular studies. The one was that we had to bring people into the research program who were able to develop serological test systems that would allow us to determine what type of bird blood the mosquitoes were feeding on. And that's when we brought in Dr. [Constantine H.] Tempelis, who is still here on our faculty. He had been educated at the university of Wisconsin and was working at the West Virginia School of Medicine. He had worked with Dr. H. R. Wolfe, who was really the leader in this whole field. Dr. Paul Kirk in the criminology department here on this campus also was concerned with identifying different types of blood for criminology work, but not for birds.

So we developed those tests, and indeed, we had a better mousetrap. For practical purposes, we could identify any type of blood that the mosquitoes fed on, and we applied these tests on a very wide scale. We were a service department in this regard for many other people in many other parts of the country. Our findings allowed us to really focus on work that could be done in the next decade to determine how effective a host different

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<sup>1</sup>C. M. Herman, W. C. Reeves, H. E. McClure, E. M. French, W. McD. Hammon. Studies on avian malaria in vectors and hosts of encephalitis in Kern County, California: I. Infections in avian hosts, American Journal of Tropical Medicine and Hygiene 1954; 3: 676-695.

W. C. Reeves, R. C. Herold, L. Rosen, B. Brookman, W. McD. Hammon. Studies in avian malaria in vectors and hosts of encephalitis in Kern County, California: II. Infections in mosquito vectors, American Journal of Tropical Medicine and Hygiene 1954; 3: 696-703.

L. Rosen, W. C. Reeves. Studies on avian malaria in vectors and hosts of encephalitis in Kern County, California: III. The comparative vector ability of some of the local *Culicine* mosquitoes, American Journal of Tropical Medicine and Hygiene 1954; 3: 704-708.

species of birds would be for the viruses and whether they would be an efficient source of mosquito infection and maintenance.

The mosquito control people were very concerned and excited about going out and controlling *Culex tarsalis*, because this was something they had very little experience on. So we had to do very extensive biological studies to see where that mosquito bred and what its life cycles were and so on. That's when Barney Brookman took that over for his Ph.D. thesis.<sup>1</sup> He'd worked with me in the original studies in the first two years in Yakima, and then he'd gone into the Malaria Control in War Areas Program of the United States Public Health Service.

#### Association with the Centers for Disease Control

Reeves: At about this time, in the late forties, the United State Public Health Service reorganized the Malaria Control in War Areas Program into the new CDC (Centers for Disease Control) program located in Atlanta, Georgia. In a congressional hearing they ran into a buzz saw one day when one of the senators said, "What's CDC doing on encephalitis research?" At that time they were doing nothing. They were criticized for it, because the senator thought it was an important problem, for whatever his reasons were; I don't know the details.

Anyway, at that time Dr. Justin Andrews, who was the director of CDC, called us up at the Hooper and said, "Look, we've got to get something on the board here concerning the CDC's working on encephalitis. The quickest way for us to do this is, would you be willing to have us enter into a collaborative agreement with you?" Now, that was sort of unheard of--for the United States Public Health Service to come to a university with this sort of a request. We said, "That would be fine. What are you going to contribute?" Dr. Andrews said, "Could you use another staff member? Could we assign a scientist to you?" We said, "That would be possible." He said, "What sort of a scientist?" We said, "We want another entomologist and probably a vertebrate zoologist." He said, "That's fine, but do you have anybody in mind?"

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<sup>1</sup>Brookman, B. The bionomics of *Culex tarsalis* Coquillett in irrigated areas of a Lower Sonoran environment. 1950. Ph.D. thesis, University of California, Berkeley.



It happened that Hammon and I in '45 had stopped in Hawaii when on our way to Okinawa, and Barney Brookman was there working on rat control and other problems for the Malaria Control in War Areas Program, which didn't make much sense. He wasn't very happy and wanted to get out. So in 1946 I was pretty sure he was available.

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Reeves: He accepted the transfer to Bakersfield and agreed to live in Bakersfield and be the director of the field station. We wanted to keep the field station open year-round, so in '46 he was transferred from the Public Health Service assignment in Hawaii to us in Bakersfield. That also gave him a chance to finish up his Ph.D. program that he'd been in the middle of when the war started. They also then appointed and assigned Elliott McClure as vertebrate zoologist. Subsequently, we had a whole string of such people who were assigned to Bakersfield.

#### Fluorescent Dust Tagging of Mosquitoes

Reeves: The Kern Mosquito Abatement District wanted to know, "How far out do we have to go and do our control? How far away from the city do we have to go; how far do these mosquitoes fly?" I said, "We don't have any idea. We could guess, but you can guess as well as we can." They insisted, "Well, we'd really like to know how far they fly."

This is one of my favorite stories. We knew tracking mosquitoes was a problem, but we didn't know how to do it. A few studies had been done where people took India ink and little fine pens or brushes, and they marked numbers on mosquitoes' backs and turned them loose. Well, that wasn't wholesale enough for me. I always believe in doing things on a big scale if we can. One night Bill Hammon and I were on a field trip someplace. We went to a movie. The detective in the movie was William Powell, who made a detective series with Myrna Loy as his co-worker. Asta was her dog; Asta's always in crossword puzzles. [laughs]

Anyway, he was a detective and was called in on this case where somebody was tapping the till, as they called it in those days. That meant somebody was robbing the cash register. The detective said, "No problem. I'll solve it for you. Just go ahead and leave the room. Next time you're robbed, just give me a call, and I'll come over and solve it." So everybody left the room, and he reached into the pocket of his dapper double-breasted

suit, got out this little atomizer, went over to the cash register and puffed this gray dust in there--films weren't in color in those days--and closed it up and left.

A couple of days later they called and said, "Hey, somebody robbed the cash register again." So he came in, took a little ultraviolet lamp out of his pocket, set it up, and he had everybody come by and put their hands in front of it. This one guy's hands just shone like a Christmas tree. He'd put a fluorescent dust on the money in the till, and this guy had fluorescent dust all over his hands. Just astounding, and I hit the ceiling. [pounds table with fist] "Bill, we can do it," I said. "I can mark mosquitoes now." Everybody in the audience was saying, "Shhhhhh!" Anyway, Bill thought I was crazy.

To make a long story short, I went into the fluorescent dust business about this time and found that there was a dye called rhodamine B. I worked out a system in which you could mix that dye with gum arabic, and then after you got that made into a paste, you dried it and then ground it up to make it into a fine dust. Then you could put the dust into a little atomizer and puff it onto the mosquitoes and put them in a humid environment. The gum arabic would absorb a little moisture, and the dye would fasten on the mosquito like glue. Every mosquito was a little bright red Christmas tree light when you put it under a fluorescent light.

So we used that dust and started doing mark-release-recapture studies on mosquitoes. You could collect a bunch of mosquitoes or raise a bunch of mosquitoes, mark them with the dust and turn them loose at a known place, and collect them at various distances away from there. If you found a marked mosquito, you knew exactly how far it had gone, because you knew when you'd turned it loose, you knew how old it was when you turned it loose, and you knew what its first and last name was when you turned it loose; so you could get a lot of information.

Hughes: And you were surprised to find how far a mosquito flies?

Reeves: They were going a mile to two miles in that first very small experiment.<sup>1</sup> Since that time we've done many studies, and now we use commercially available fluorescent dusts. We have a wide array of different colors. We have red, blue, green, yellow, and silver. So we can mark different mosquitoes with different colors

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<sup>1</sup>W. C. Reeves, B. Brookman, and W. McD. Hammon. Studies on the flight range of certain *Culex* mosquitoes using a fluorescent-dye marker, with notes on *Culiseta* and *Anopheles*, Mosquito News 1948; 8: 61-69.

at different times and so on. Now we get a lot of recoveries of this mosquito at distances of between five and ten miles. It's been done with other mosquitoes that go over twenty miles within a twenty-four to forty-eight-hour period.

Hughes: What's the lifetime of a mosquito?

Reeves: We can answer for people what their life expectancy is; it's called a life table. Now we can do the same thing for mosquitoes. To make a long story short--and we can get into more detail later --we have found in the Central Valley of California that if we mark mosquitoes and turn them loose, up to 25 or 30 percent die per day or leave the area. That means that if 25 to 30 percent die per day, and you get mosquitoes back a week to two weeks after you've turned them loose, you've turned a lot of mosquitoes loose in order to still be able to collect them. I've done a study with Bruce Eldridge with snow mosquitoes in the Sierras and only had five percent mortality per day and got mosquitoes back six weeks after we turned them loose. We recaptured almost half the mosquitoes that were turned loose, which is almost impossible to believe. Fluorescent marking is an accepted general technique we use now to determine the life table.

When you say, "How long does a mosquito live?" it depends upon how hot it is; it depends upon all the hazards that it's exposed to. But within a period of ten days, less than 5 percent of the *Culex tarsalis* mosquitoes that were there originally will still be there. It takes around eight to ten days for an encephalitis virus to be incubated and transmitted. So you can see that you have to have a very large total population of mosquitoes in an area for a virus to maintain itself.

These are the sorts of by-products that come out of the research questions that come up. The mosquito abatement districts needed to know where this mosquito comes from and how far it goes, and we gave them an answer: "This is the sort of water it breeds in. This is where you should focus your attention." We were able to tell them, "If you want to do encephalitis control, focus your attention on *Culex tarsalis*, and don't waste your time on all the other mosquitoes. There are twenty-two species out here in this area, and one of them is your target. If malaria is your target, it's *Anopheles freeborni* in this area." Different mosquito species are so different in their preference for what water they'll breed in and so on that you can really help a mosquito control program to focus its attention so it can be more economical and more effective.

Hughes: And they listen to you?



Reeves: Some do, some don't. Some don't believe this. Some do. We showed that the vector can be controlled--that is, can be reduced in numbers--if you focus your control program. We can assess the amount of virus that's active in the mosquitoes by making routine collections of mosquitoes and testing them for virus and get different types of viruses out of them. We can use birds as sentinels to see what proportion of birds are getting infected in a time period. We can do it by catching, marking, and recapturing wild birds. We can do it by putting out sentinel chickens and bleeding them periodically.

Hughes: Was the use of sentinel chickens an established technique?

Reeves: We established it at that time. So you can now develop a surveillance system for these viruses, and you can have surveillance of the human and horse cases that are occurring if you get the medical and veterinary professions to cooperate and submit diagnostic specimens. In this period between 1940 and 1950, these basic methodologies and knowledge were developed.

#### Collaborating with the California Department of Public Health

Reeves: At this time we were very fortunate in California, because this state has been very progressive. We had people like Governor Earl Warren and legislators who were very progressive in supporting this sort of thing. We were in the process in this state at that time of developing what was probably the outstanding health department in any state in the nation. At the end of the war they recruited an unusually competent group of people into the state health department. Dr. E. H. Lennette came in as the director of the state virus and rickettsial disease laboratory; they brought in a series of people in the infectious disease field--Dr. Robert Dyar, Dr. A. C. Hollister, and Dr. Lester Breslow. Dr. W. Allen Longshore, whom we recruited onto our faculty, moved to the state health department in 1950 because of the loyalty oath. We had indoctrinated him into encephalitis virus research with us, working in Kern County in the forties.

The California State Department of Health in the postwar period also developed a very strong vector control section. After the retirement of my friend Mr. Gillespie, whom I had some argument with about my mistakes in DDT use, Mr. Ed Reinke and Mr. Frank Stead came in successively as head of the environmental health and engineering program and developed a very strong vector control section. First they recruited Mr. Arvie Dahl, who was an engineer type. And then Mr. Richard F. Peters, who was one of my

classmates in entomology, came out of the army and took over the vector control section, and he really became the leader in vector control program development for the state health department, which led to rapid expansion of local mosquito control districts.

So we had a really outstanding group to work with there, people who were scientists in their own right and who made great additions to the knowledge that we'd accumulated and made sure it got applied in the field. Actually, it set the scene for development of a surveillance system in which we could monitor how much virus was present in California and use this information for action in control programs. That's why I call this the discovery decade, because we didn't know where we were going when we started, and then it all worked out.

### Facilities##

[Interview 4: January 11, 1991]

Hughes: Dr. Reeves, last time we discussed the work in Washington and California, and there remain a few questions--in my mind, anyway. One of them is to describe the facilities that you had in Kern County.

Reeves: The facilities in Kern County became available in a very peculiar fashion. When we started working there in 1943, there were just three of us, Pedro Galindo, Bill Hammon, and I. We had no difficulty getting the Kern County Health Department, which was housed in Kern General Hospital, to give us a table in the basement with a couple of chairs and a place we could plug in microscope lights. We did all of the work in that basement.

When we were getting ready to start our work in 1945, we realized that we were going to have a considerable increase in the number of people who were going to be working there. That's when we were preparing to do the first DDT experiments. The health department told us that they couldn't accommodate us; they had no more room, and they'd actually expanded the health department into the basement where we'd been. So they were sorry, but they couldn't collaborate with us for space and suggested I talk to the medical director of the Kern General Hospital.

So I went and talked to him. He said he was very sympathetic. They certainly wanted to continue their collaboration, because we obviously were not only working with the health department, we also were working with the hospital on the

infectious disease ward, getting samples for diagnosis from encephalitis and polio cases. They were very anxious to collaborate, but he didn't know of any space that was available on what they called the campus, which included an old folks' home, a TB sanitarium, and all sorts of outbuildings. He said if I could find any space that would serve our purpose, I'd be welcome to it, but he didn't know of any.

So I went out and started prowling around the grounds. Way in the back of the grounds I found this old adobe building that had been built by the WPA. Well, it wasn't really that old, from just the thirties. The WPA had built a series of adobe buildings on the grounds that were being used for an old folks' home and so on. I found one that was completely unoccupied. As a matter of fact, I found two that were unoccupied. They weren't large buildings. They were, oh, I would say 30 by 20, maybe 40 by 20-foot buildings. There were a bunch of old hospital beds and a lot of dirt in them and no people.

I went to the hospital director and said, "I found two buildings that would be very, very good for what we're doing. They are right next to the back road that comes into the hospital grounds. If you let me have them, that would be wonderful." He said, "I have to see the buildings. I don't know of any empty buildings." So I took him on his own campus, and I said, "These are the buildings." He said, "Oh, but that's a filthy dirty place, and they are old buildings built by the WPA." I said, "Look, you said if I could find any empty space I could have it, and it certainly looks empty to me except for cots and for dust." So he said, "They are yours as long as you need them."

I said fine, and he got me a key for the locks on the doors. I stripped down to my working clothes and took my necktie off that I had put on just to see him. That was a very official meeting when you put a necktie on in Kern County. [laughter] I got a hose, and I just washed the places out. It was like the Stygian stables; they were full of dirt, and I just washed it all out the door. Fortunately, they had concrete floors and adobe walls. I washed out a ton of dust and made mud out of it, and the people from the hospital came to take the cots and put them in the county dump; I had no use for them. So I wound up with these buildings. Each had two nice, large-sized rooms, and I'd say I had over one thousand square feet of space; I don't know. They were good-sized rooms, and I also had two bathrooms and showers.

Then I bought some paint and repainted it all. I was doing all this by myself; there wasn't anybody with me at the time. The people from the hospital had a work crew and a shop, and they came over and built some cabinets for me and gave me some old



furniture. So there we were with buildings, rent free, utilities free. I realized how ephemeral these things might be, so I asked the business manager if we could get a letter of agreement from the Kern County Board of Supervisors. He thought that would be a good idea, because that would relieve him of any responsibilities. So we got an open letter of agreement from the board with copies to the regents and the Berkeley campus, saying that the buildings were ours as long as we wanted them.

We carried out that summer's activity, which to us was a big collaborative program. The Yakima project had been collaborative in the sense that we were from the Hooper Foundation and we went there on the invitation of the Washington State Health Department. We worked with the local health department and also with staff from the microbiology department of Washington State University, as they were collaborating in the laboratory, testing serological samples. Our quarters in Yakima had been very simple. We rented a big two-story house, and we worked in the basement and lived upstairs, and that was a very primitive laboratory. In Kern County we finally had buildings that were ours, so we could make modifications.

### Staff Expansion

Reeves: It was shortly after the 1945 project in Kern County when we decided to establish a permanent staff in Bakersfield and not to just be traveling back and forth from San Francisco. So in 1946 I took advantage of the fact that the second building next to ours was still unoccupied. It was a smaller building, but it was still usable. We got that included in our agreement, so now we had two buildings. I modified one room into an insectary. That really was the development of what we later called the Arbovirus Field Station. Subsequent to that, beginning in 1946, we really developed large-scale collaborative programs. The 1945 project represented collaboration between the state health department, the hospital, the mosquito abatement district, and us. The next year's project became even larger than that. I think we wound up with eighteen or twenty people in Bakersfield.

Hughes: Why don't you tell me who was there from the outset and how that staff was augmented?

Reeves: The staff to begin with in 1943 was Pedro Galindo, Bill Hammon, and me. In 1945, when we did the DDT project, we had a collaborative agreement with the California State Health

Department, environmental health section. They financed the DDT project.

Hughes: Is that something you arranged?

Reeves: Yes.

Hughes: By doing what?

Reeves: Well, we were under pressure from them, the California State Legislature, and the local public health people to demonstrate that control of encephalitis could be carried out. The spraying of chicken houses and other buildings with DDT was our first effort to show we could control encephalitis. We failed but advanced our knowledge. I talked about that before.

I was by myself. So in 1945 I told them, "I need a crew to help me spray the twenty-eight square miles of Kern County we're going to spray with DDT." They said, "Fine. How many people do you need?" So I got some people assigned from them, which actually represented a plague control crew that usually was out shooting rabbits and ground squirrels to collect fleas to see how widely plague was disseminated in California.

Hughes: Did this crew have any scientific background?

Reeves: None. These were high school-level people. They were very good with guns, very good at shooting squirrels. As a matter of fact, their procedure on a plague survey was that each guy would check out a box of shells, which would be twenty shotgun shells, and put ten cents in the kitty for each shell. Then for every animal they brought in, they'd take out ten cents. Their game was to see if they could line up two or three squirrels and get them at one shot so they could make money if they got back to the car and got the money out before the other guys did. They were very good in the field and very, very durable people physically. They also sent one engineer down; I don't quite know yet what he was supposed to do, but we made a good spray man out of him.

As I said, the Public Health Service had what they called a Malaria Control in War Areas Program, the organization which preceded the present Centers for Disease Control of the U.S. Public Health Service. The MCWA person was a fellow by the name of Ed Washburn--G. Edwin Washburn, to be more specific. He was an entomologist, and he was in uniform and was working on assignment to the State Health Department. He really didn't have a lot of malaria to control because there wasn't any malaria in California that year. So they assigned him to me. He represented another



collaborative aspect of the program. In the postwar period, Ed became the manager of the Turlock Mosquito Abatement District.

He and I exchanged places every week through the summer of 1945. I'd be in Kern County one week, then I'd go back to San Francisco to teach the medical students about tropical medicine, and he would be down in Kern County. Then the next week he would come up to Berkeley to carry out his malaria control work, and I'd be down in Bakersfield. We would meet down around Turlock someplace on old Highway 99 and exchange information about what had happened during the week. So he'd be there collecting mosquitoes and identifying them one week, and I'd be there the next week, which worked out very nicely.

The next year, in 1946, we really expanded the program, because we had gotten into a number of new projects. The state legislature's primary concern at that time was to prevent malaria from being reintroduced into California after the war by the infected servicemen. We had convinced them that we should enlarge that program to include an encephalitis study and control program. So the State Health Department now had money from the state legislature for demonstration purposes and for malaria and encephalitis research. So again we entered a collaborative agreement with them to carry out such research.

#### Collaborating with Various Agencies

Reeves: I talked earlier, in the section on surveying wild birds, about collaboration with Carlton Herman of California State Fish and Game and recruiting Elliott McClure. CDC also agreed that they would pay for a technician at the field station. That's when I recruited Eva French, who was a nurse. Originally I thought I'd have her chasing cases up in the hospital, but it turned out that we made a very good bird malaria technician out of her. We'd become interested in bird malaria, which seems a peculiar thing, but our problem basically was that we didn't know what birds *Culex tarsalis* was feeding on. We wanted to know the answer, and we didn't have a precipitin test to separate different species of birds well enough that we could identify blood meals in a mosquito's stomach.

So suddenly we had developed a burgeoning unit in Bakersfield. We did a very large study on bird malaria that went on for several years. As I mentioned, one of the papers was by McClure. We found seven different species of malaria and high infection rates in the birds. We found that *Culex tarsalis* was



the primary vector, and this answered our question about what birds mosquitoes fed on, because the birds that had malaria had been fed on by *Culex tarsalis*, and the other mosquitoes were not as important as vectors. As you can see, at this stage we were developing a constantly increasing staff.

The Kern Mosquito Abatement District originally had been called the Dr. Morris Mosquito Abatement District and had been developed to control human malaria. This disease was a real problem in Kern County back in the early 1900s. The board of trustees of the district came to me because the manager of their district, Fred Hayes, had died of a heart attack, and they were in the business of recruiting a new manager. They asked if I had any suggestions.

I didn't waste any time saying I certainly would have some suggestions, because we were happy to work with the mosquito abatement district, and they were carrying out an expanded control program on encephalitis that we had recommended. That's when I recommended that Arthur F. Geib would be an excellent candidate. I knew him very well; we'd grown up together in Riverside. We had played basketball together, and his sister had married a guy who lived on a ranch near us. One of his brothers had been our student body president in high school. Anyway, I knew his family very well. Art was a very bright guy, and he'd gone through the sanitarian training program in our Department of Hygiene, which preceded the School of Public Health. During the war he'd worked on the mosquito control aspects in the Malaria Control in War Areas Program with the State health Department. I recommended the district recruit him, and they did. It was a fantastic choice, because we knew each other very well. He was a very forward-looking sort of a person with reference to new methods for mosquito control.

After that appointment, any time that we needed to have collaboration with the Kern Mosquito Abatement District, we got it. For instance, if I wanted to do a flight range study on *Culex tarsalis*, Geib wouldn't hesitate at all; he would assign four or five people for a time period to work with me on mosquito collecting and this sort of thing. In addition, it seemed like every time we turned around we would find some other group that was interested and wanted to collaborate. We had the Kern County Health Department, Kern General Hospital, Kern Mosquito Abatement District, the County Board of Supervisors, U.S. Public Health Service, California State Health Department, and California State Fish and Game Commission. That's a pretty extensive group of agencies. Now, the problem was to put the people together and make a team out of them, and for them not to say, "I work for So-

and-so," not for you. They were working for the unit, and that was my philosophy and rule.

Hughes: Was it your job to see that they cooperated?

Reeves: It seemed that way. [laughs] There was no messing around about it. We had times when somebody would say, "Well, I work for So-and-so." I'd say, "Okay, let's settle that right now." I would just pick up the phone and call their boss and say, "Your man down here says he works for you, and he's differing with me about this, that, or whatever." The answer that I received always was, "Put him on the phone." They'd get him on the phone and say, "What Reeves says is it. Don't argue with him."

Hughes: So there weren't ongoing problems?

Reeves: No problems. Actually, it was a very gratifying sort of a situation, because working in a rural community like Kern County you had to have your contacts with the people who counted in that community. If you had something that came up as an issue, you had to make sure that if it threatened to become a political thing, you'd have support from key people in that social unit. If you needed to have the County Board of Supervisors understand what was going on, you had to have local people who knew and would tell those people. It wasn't my job to do it; I wasn't electing them.

#### Testifying at Kern Mosquito Abatement District Hearings

Reeves: I'll give you a simple example of that. The Kern Mosquito Abatement District, starting in 1946, was having hearings on annexations to their district. A mosquito control district is a peculiar sort of a political beast. It's a state law that citizens in a local area in California can develop a mosquito control program if the taxpayers want it. If they want it, they have to agree that they will tax themselves for mosquito control and create a district. It's like a water district or one of the other special districts; it's a separate thing from the other elements of the local government. Something like 30 percent of the taxpayers in the area to be annexed have to sign a petition that that's what they want, and then they have hearings about whether this should be done or shouldn't be done.

There would be a group of people wanting to annex, say, five hundred square miles of Kern County to their mosquito abatement district, so the district's trustees would have hearings. I occasionally was asked if I would come as a witness to testify why



it was important to annex that area to the district. At the same time, the local health department would be asked if they could send somebody to give the health department's side. At the same time, the state health department would get a similar request to send somebody.

Well, on this occasion the local health officer called me and said, "Bill, are you going to be in town on this particular day?" and I said, "Yes." "Would you be willing to go in my place and represent the health department?" and I said, "That's all right; I'm going to be there anyway." "Oh, that would be great." The health officer from the state also called me and said, "Bill, can you go to Bakersfield to testify for me at the hearing?" So I wound up representing four different viewpoints: two health departments, the mosquito abatement district, and myself, because I also was interested.

Then I found that I was in a peculiar situation in which oil companies and other unusually large landowners had sent this battery of lawyers to argue against the annexation because they didn't want to be annexed and pay the tax. It was the local people who lived there who wanted to be annexed, not the big outfits. So they sent their lawyers from Los Angeles to fight it.

Hughes: Why did they care?

Reeves: They didn't want to be taxed. And they didn't live there. If you have a hundred oil wells out in the middle of the area, as far as you're concerned that's a money-making proposition. The mosquito abatement district wants it annexed because that's the most valuable tax land and is producing mosquitoes. The Kern Land and Cattle Company that owned half of Kern County didn't want to be taxed.

Hughes: When you were wearing these four hats, was there any stipulation about what viewpoint you were expected to put forward?

Reeves: I was supposed to present and represent the scientific facts about the importance of mosquito-borne diseases and pest mosquitoes, period, and tell why this was a public health problem in Kern County.

I'll never forget this hearing. The lawyers had gotten up and made their case, that there were no mosquito problems in Kern County and so forth; there were no mosquito-borne diseases in Kern County.

Hughes: How could they make such a statement?



Reeves: It was easy. They just got up and said it. They didn't know anything about it. Lawyers from Los Angeles were representing these companies, and their job was to prevent taxation on those properties; that was their job. So I sat there developing a head of steam the whole time. As far as I was concerned, these guys were saying that the sun won't come up tomorrow.

Finally the lawyers ran out of things to say, and the president of the board of trustees, John Fox, whom I knew very, very well and had been working with very closely for the mosquito abatement district, said, "Dr. Reeves is here, and he's representing the University of California project. He is working in Kern County on these diseases, and I understand the health officers for the county and the state have asked him if he would come and give testimony for them. Dr. Reeves, do you have anything to say about this?"

So I got up, and I didn't make any bones. I just said, "It happens that we've just had a big epidemic of encephalitis in Kern County, and we have unequivocal evidence that mosquitoes are carrying it. It's also an important problem in horses, and I can be very specific. I can tell you that the workmen working on one of your oil rigs walked off the job because the mosquitoes were so bad they wouldn't work there anymore. I can also document places where people who were out harvesting crops refused to work anymore, and you couldn't pay them enough to keep them, and I've got the actual entomological evidence of this. I mean, I've got extensive records of how many mosquitoes are there, and I can also tell you where those mosquitoes came from and what diseases they transmit."

I got about this far, and all of a sudden one of the lawyers got up and said, "Mr. Chairman, I object to the doctor giving testimony." The chairman said, "Why?" He said, "I don't believe he's a taxpayer in Kern County. He has to be a taxpayer in Kern County to be giving testimony at this hearing." The president of the board said to me, "You're not a taxpayer, are you?" I said, "I spend a lot of money here, but I don't own any property." He said, "If you don't represent a taxpayer, I guess that the lawyers have you on a technicality. They represent a taxpayer, but you don't."

About this time, a little man in overalls in the back row got up--a real little dirt farmer. I knew him very well. He was from out at Buttonwillow. He stood up and said, "I'm getting sick and tired of this mess. I need to be represented, and I can't give the testimony; but Dr. Reeves can, and he is my representative." [laughter] "I'll even pay him if I have to." About this time one of the rich farmers in the area, a fellow whom I knew very well,

got up and said, "And he can represent me also." The chairman smiled, turned to the lawyers, and said, "You guys satisfied now?" They had nothing more to say.

Hughes: Wonderful.

Reeves: So they annexed the area. Now, that was collaboration. That represented my payback for what we were getting from the local people. The interesting thing was, after that was over and they adjourned the meeting, one of these lawyers came up and said, "Hey, how about having a drink with us guys?" I thought, "God, these guys are going to kill me or something." I said, "Well, I don't think I'd better." "Come on," he said. "We want to know more about this stuff. You know, we don't know anything about this, and we're interested. It's important for us to know enough about this to really be better lawyers." So I went and had a couple of drinks with them, and they were very nice. They said, "You know, we fight with each other all the time. Don't take this personally." I said, "Okay, I guess I've learned my lesson. I won't take lawyers personally after this."

#### Collaborating with the California Department of Public Health

Reeves: We carried out collaboration of that type with any organization if it was mutually advantageous to them and to us. At that time the state health department was just developing its laboratory diagnostic programs on diseases like encephalitis. That's when Dr. Lennette had just come here in 1946. Because the war was over, the department was increasing its staff in the laboratory, communicable disease, vector control programs, and so on. Also at that time the state legislature had allocated a million dollars to the state health department for supplementary money to be used in support of mosquito control and for mosquito control demonstrations in the state. That's when they first organized the vector control branch.

Hughes: That was mainly with malaria in mind, wasn't it?

Reeves: It had shifted almost completely to encephalitis, because there was no evidence that malaria was becoming a major problem at that stage. Vector control still encompassed malaria if necessary, but there was no malaria control to be carried out, because we didn't have current evidence of transmission. We've had some in more recent years.



Encephalitis and plague were the two vector-borne diseases that were of major concern to the state. They also were concerned with relapsing fever, a tick-borne disease that occurred in cabins up in the Tahoe area and places like that in the high mountains. They already had a plague surveillance program which Dr. Meyer had been instrumental in developing, and they were out collecting animal blood and ectoparasites like fleas to test for plague organisms. They also were broadening the program extensively beyond plague and relapsing fever and possibly malaria to combat the problem of encephalitis. They finally knew enough about encephalitis from our research to begin to develop an extensive program.

Hughes: When and why did laboratory work shift from the Hooper Foundation to the State Department of Public Health?

Reeves: Why and when are both very easy questions. Beginning in the late 1940s, we knew that we couldn't go on doing routine diagnostic services at the Hooper Foundation or at the School of Public Health, which was just then developing, if we went on doing nothing but the diagnostic services of accepting blood, feces, and autopsy samples from sick or dead people to prove whether they had the disease or not. It never would be accepted as a research project. It clearly was a service.

Hughes: By the University?

Reeves: By the University and by financing organizations such as the U.S. Public Health Service and the army. Once the methodologies had been worked out, or once the viruses were known, then that became an important service activity. It was a convenience to practicing physicians, health agencies, and mosquito abatement districts to know how many cases there were or if virus was present in mosquitoes, but it was no longer a research program as it had been. It was important to us to have this information for epidemiological purposes, to relate to different things like mosquito population size and where a virus was and so on. But we were anxious to shift away from the routine diagnostic service. It still was a very important service, and the State Health Department laboratories had an obligation to assist physicians, veterinarians, and health departments in diagnosing the diseases that were present in the state.

So at that time that activity was shifted to the laboratory section of the Department of Public Health. Lennette was very interested in having this as a part of his responsibility. For a while we continued to do the diagnostic work for Kern County but not the whole state. Within a year or two we also shifted that activity to the state, because it was obvious that our



methodologies and findings and his were absolutely identical, and there was no reason for us to keep the data as part of our program.

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Reeves: As a further example of a shift in activities, in 1949 we had Dr. W. Alan Longshore, Jr., whom we had recruited from New York to our faculty in epidemiology. He became interested in working on encephalitis with me in Kern County. As I mentioned, when the loyalty oath came up at the University within a year or so after he came, he didn't want to take the loyalty oath, as he was a Quaker, so he left us and went to the State Health Department. So now they had a person who was well oriented on the epidemiology of encephalitis in the communicable disease section of the State Health Department, and that was very complementary to the other things that Lennette's group was doing in the laboratory.

Ironically, the loyalty oath became required of all State Health Department employees shortly after that, so Dr. Longshore decided he would finally sign the thing. He stayed, and he was a very valuable person who worked closely with us for years.

#### Medical Students on the Field Team

Reeves: Another thing which we started doing in that 1946 collaborative program was taking medical students onto the field team during the summer. The first two students we took were very interesting. They had taken our tropical disease course at the medical center at U.C. San Francisco, and they came to us in the early spring of '46 and said, "We found this very interesting. We have talked to Dr. Meyer about our interest, and he suggested we come and talk to you. Would it be possible for us to come to Kern County and spend the summer just to learn what your field of research is about?" I said, "Well, we don't have any money to pay you," because at that time we didn't. I was fishing around getting collaboration, which didn't cost us anything. The students said, "We don't want to be paid. We want to learn." Now, that was a very unique experience. It's not really that unique, but it's unique. So I said, "Fine, but you just have to do whatever is there to be done." They responded, "Oh, that's fine."

One of their jobs was to keep an eye on the communicable disease ward in the hospital. At that time polio, encephalitis, and other such diseases had to be put in isolation because it wasn't known if these diseases could be transmitted from person to

person. So each day one of them would go to the communicable disease ward and see if any new admissions of encephalitis or polio had taken place and get the laboratory specimens we needed and the history for those cases. But then the rest of the time they were free to work on anything else we were doing.

The bird malaria study was a big project at that time. We were dissecting thousands of mosquitoes to get the salivary glands and the stomachs out and to see if they were infected. We were doing thousands of blood samples from birds that all had to be collected and smears made of them and examined for parasites. So we made instant experts on bird malaria out of those two medical students.

The outcome was interesting, because one of these students was Leon Rosen, who subsequently became one of the real leaders in studies on the epidemiology of many infectious diseases. Later I was able to get him recruited to a position down in Tahiti to study filariasis. He became one of the real experts on mosquitoes in transmission of filariasis as a project out at the University of Southern California with Dr. John Kessel. John had already recruited another person, Dr. Henry Bye, for that group, whom I recommended, who had formerly been the biostatistician for the Kern Health Department and then had gone on to medical school at USC. Henry was doing the medical aspects of filariasis, and they wanted someone to do entomology studies in Tahiti. I convinced them that Leon Rosen was a physician, but he also knew a lot about mosquitoes, which he had learned working with us. His first scientific publication was with us on bird malaria and experimental transmission from bird to bird with mosquitoes. He's still very proud of that paper.

He's gone on with work at the National Institutes of Health in Bethesda on diarrheal and respiratory diseases in infants in orphanages, and then he went out to Hawaii and has done extensive and very, very excellent work on dengue fevers and the complications of hemorrhagic fever and shock syndrome. He finally retired this last year. Now he's spending half of each year at the Pasteur Institute in France doing research and half the year in Hawaii. So here's a person who started as a medical student with us and went on to spend a whole career in the field.

The second medical student became one of the outstanding clinical psychiatrists in southern California.

Hughes: Who was that?

Reeves: That was Ray Herald. Ray decided that he didn't like the nitty-gritty, day-by-day hard work entailed in this sort of research,



and that was fine with me. If we accomplished that, we accomplished a lot. Some years later the State Health Department was trying to establish a mental health research program in the Pasadena area and getting nowhere with the psychiatrists in the local group, just absolutely coming to a dead end. They talked to me some about it at lunch one day, and at their next meeting in southern California, Ray Herald showed up at the meeting. It happened that he was one of the leading psychiatrists in Pasadena.

He got up and read the riot act to his colleagues: What's the matter with them? Didn't they know how important research in epidemiology was? Really gave them a blast and completely swayed the audience to change and become very collaborative and cooperative with the State Health Department. I knew where Ray was, but I hadn't had any contact with him. His training in epidemiology in Kern County came through and was a real payoff. The people in the State Health Department said, "Boy, this guy really knew what he was talking about with reference to epidemiology." We kept up a correspondence for a while, but he now lives in Panama. I've lost track of him; he married a Panamanian girl and moved down there. I don't know what he's doing now.

Anyway, those were our first two medical students. We've had literally dozens of them since that summer.

### Applied Versus Basic Science Research Goals

Hughes: What was more important to you in your research, the purely scientific or the practical?

Reeves: I don't think we ever made any division between purely scientific and practical. We had picked people to work on this project who weren't particularly concerned with going off on some real theoretical aspect of the research. We developed or applied a lot of new methodologies to get answers that had very practical application. We developed a lot of new methods that are routine now in this field.

But if you're talking about basic research in a "science for science's sake" sort of thing, we haven't done much of that. Anybody on our staff who wanted to go that direction got discouraged very strongly and usually moved, the reason being that the collaborators we had, the sources of funding we had, were interested in knowing how this disease spreads. Most people would call that applied practical stuff. So anything we did, we wanted



to have a possibility of practical utilization. It didn't mean it wasn't good science.

As an example, we isolated a new virus, California encephalitis virus. Immediately, some of the people in the laboratory and in the field wanted to do nothing but study California virus. It was a new virus. Our reaction was that if we couldn't show that it was an important cause of disease, we couldn't justify our spending a lot of time on it. When we could only find three human cases and had worked out the basic biology--that it was carried in rabbits and by *Aedes* mosquitoes and that it wasn't of any particular importance disease-wise to animals or to people--we just put the virus in the deep freeze.

There wasn't a renewed interest in California virus until years later, when they found LaCrosse virus in Wisconsin and it turned out that LaCrosse virus was very closely related to our California virus, and there was disease associated with LaCrosse virus. California virus no longer was a laboratory curiosity. People were interested in the relationship of this virus to a number of new viruses. California virus, was the first virus in a new group of viruses that was named the California virus complex. It also was important in Europe, public-healthwise. They're still important in the northern and northeastern part of the United States, but up to now still are not of any real significance in California, so we've done very little with it. Currently we have found this virus or a closely related virus in salt marsh mosquitoes down around Morro Bay. It's probably not the same virus, and it's in a different mosquito there. We're pursuing the new finding to see if it has any significance healthwise in the coastal environment.

A whole series of new viruses were isolated in Kern County by us and others, and these have immediately gone into other laboratories at the Centers for Disease Control and the World Reference Center on Arboviruses at Yale. They're very valuable there for showing how widespread these various viruses are and what their relationships are to each other. We haven't done that. It's not our game.

Hughes: Would you say that reflects your personal orientation?

Reeves: It undoubtedly does. I think any project of this type reflects the philosophy and the psychology of the people who run it. I think that was true in Dr. Meyer's case. You didn't find a lot of what you would call basic science ever being done at the Hooper Foundation, although it is today, as that program now is focused on chronic diseases. It's almost all on cancer research, and most of that is "pie in the sky" in the sense of practical applications

in an immediate, obvious way. Dr. Meyer oriented the Hooper Foundation towards diseases of nature transmissible to man, the zoonoses. It also became my primary interest. Hammon had no difficulty adjusting to this as a philosophy and as an objective. But actually we've had people who have even left our organization because they couldn't pursue some other theoretically very exciting things. Now, as we have developed academic degree programs in microbiology and epidemiology, we certainly have had students who have gone into some very basic research. In such instances, we consider the students' research as being peripheral to the basic theme of the overall field research program.

### H. E. McClure and His Mourning Doves

Reeves: I'll give you an example of how an individual interest can potentially upset the whole program. We were doing extensive bird studies; thousands of birds were being collected. We were not only interested in malaria but were taking blood samples from these birds to see if they'd had encephalitis virus infection. They usually don't get a disease; they just get the infection, and they were a source for mosquito infection. Dr. McClure, whom I mentioned earlier, was a really hard worker; he was getting thousands of blood samples. One day we were reviewing the samples and what proportion of birds had antibodies to encephalitis viruses, and I realized we were getting very few samples from mourning doves.

Now, the mourning dove happens to be one of the most common birds in California. There is an open hunting season on them because people like to hunt them, and over a million of them are killed a year. There's no shortage of mourning doves, is what it amounted to. Dr. McClure had done his Ph.D. thesis on the mourning dove, so he was the world's expert on mourning doves. Not many people had done their Ph.D. thesis on this bird. That's one of the reasons I hired him; he'd worked on pheasants and mourning doves.

Every time I was in the field I'd see nothing but mourning doves all around. But we weren't getting any samples on mourning doves. I said, "Mac, we're not getting samples on mourning doves." He said, "Oh, they are awfully hard to catch." I thought this was sort of weird, but I let it go for a couple of days. One day I said, "You know, Mac, I'd like to review your bird-banding records," because every time he caught a bird for any purpose, he put an aluminum band on the foot that had California Fish and Game and National Wildlife Federation identification numbers on it. If



any of those birds were caught again or shot, the bands were turned in, and they were sent back to the person who originally banded them to give him a record of this recovery.

So we started looking at banding records, and he had hundreds of doves that were banded. They were banded in the nest, they were banded when trapped. I said, "Mac, you've got to stop doing everything you're doing right now. You have to write a paper that's really going to be exciting and new, and nobody else knows how to do it." He said, "What's that, sir?" I said, "How in the heck do you get a band on a mourning dove and not be able to get a blood sample? That must be difficult. Do you flip the bands in the air? I mean, how do you get them on? [laughter] You don't catch them, obviously, because you told me two days before that you can't catch them." He just looked at me. He said, "I love them so much, I just can't bleed them or kill them."

Hughes: Isn't that something?

Reeves: I said, "That's too bad. It really is leaving a big hole in our data." I didn't argue with him any more. So he went off to do his thing, and I just went over to the cabinet and took out a twelve-gauge shotgun, a box of shells, syringes, needles, and tubes. I went out to where his records said he'd been banding all these birds, and I shot twenty birds and bled them, brought the blood samples in, and brought the bands in. I went over to Mac and I said, "Mac, I've got a bunch of returns for you on bird banding. Here." I dropped about ten bands on his desk. He looked at them and said, "Where did you get those?" I said, "I just shot them." "Ohhh," said Mac.

I was ruining everything that was dear to his heart. I was ruining all of his studies on doves. I said, "There's one way to stop this. You stop turning them loose when you catch them until you have taken a blood sample, and I'll stop shooting them. Until you do, every week I'm going to go out and shoot birds. I don't want to, but that's the only way we can get the blood samples, and that's our job. That's your number-one job, blood sampling. The other things are hobbies. They're not your job." We had no more difficulty. We got a lot of dove bloods after that, and he still could turn them loose after he took a sample.

That's a roundabout way to answer your question, but we were dedicated in the sense of what our audience wanted. Our audience out there was the health departments, the mosquito control districts, the physicians, and the people in the community. Our sources of funding were from the state fish and game, U.S. Public Health Service, U.S. Army, and none of them were interested in what you might call basic research in the sense of theoretical



research. They were interested in getting answers to how the disease is transmitted, how it might be controlled, what the risk factors are for people getting the disease, what the natural cycles are. And that's what we have dedicated ourselves to ever since then. I think that's what we're known for.

### More on Mosquito Control

Hughes: You mentioned at the beginning that you had decided that twenty-eight square miles was the area that you had to control for mosquitoes. How did you arrive at that figure?

Reeves: We didn't decide that twenty-eight miles was what we had to control. We controlled as large an area as we could afford, and as large an area as we had materials, personnel, and equipment to cover. We didn't know how large an area we had to control. Actually, what we were doing was trying to see if we could significantly reduce virus activity in whatever size area we could manage.

Now, this obviously left questions that we knew we didn't have the answers to. One of the projects that the State Health Department paid us to do was to determine how far these mosquitoes would move and how extensive an area would have to be included in a control program to control one of these diseases and prevent its entry into towns and cities. But that's something that no one knew how to do until we started doing it.

We did our first experiment in 1944 on mark-release-recapture mosquitoes to see how far they flew, but it was a very poor study and very limited. It didn't have the same objectives; it was just a first crack at seeing, if we marked mosquitoes and turned them loose, how far away could we catch them. We found out this was a terrible way to make a living, because it was a lot of work. When we finished that project, which for practical purposes two of us did, we had released a few thousand mosquitoes, and we had got a few recaptures. We showed they'd traveled a couple of miles, and that was an interesting new fact.

Hughes: This was using the dust, or not?

Reeves: Yes, the first use of a fluorescent dust was in 1944. I swore at the end of that project I would never do such a project again unless I had twenty people to help me, just because there's so much legwork, and you have to do such an intensive sampling over a large area to get significant results.

One of the first things that the State Health Department was going to fund us to do was to extend those studies. We made darn sure that we would be able to do a large enough area to begin to get significant answers to the question of how far these mosquitoes might go. If you have an urban center like Bakersfield that you want to protect from encephalitis, it's very important to know whether you can go only a mile outside of that area and stop your control, or whether you have to go ten miles outside. We didn't know the answer. So we got support from the State Health Department moneywise to do that sort of study and extend it. That's when we learned that if you didn't have at least a five-mile or more band of protection around the area you wanted to protect, you weren't even going to touch the problem.

Hughes: Because that's the distance the mosquitoes fly?

Reeves: Not the extreme distance, but the mosquitoes will move effectively and carry virus at least that far.

Hughes: And that was a surprise, wasn't it?

Reeves: We had thought that this mosquito was not going to move that distance. Actually, we later found that we could recover them nine and fifteen miles away. Whether a significant number are going to go that distance in some circumstances is another problem. If a few mosquitoes go ten to fifteen miles, it doesn't make much difference, but if a lot of them can go that far, it makes a difference.

Hughes: What about prevailing winds?

Reeves: People assume that mosquitoes are going to go downwind; you know, the winds are going to pick them up and move them. But we find that when we do mark-release and then do recovery collections in all different directions, that a fantastic number of mosquitoes go upwind. That didn't surprise me, when I stopped to think about it. Because what are they trying to find when they're flying? They're trying to find a food supply, namely a source of blood. You don't smell food supplies that are downwind. If you're in Fairfax and you smell a barbecue, you know damn well that it's going to be upwind, right? You can smell it, can't you? How do you know it's a barbecue?

Hughes: Experience, I guess.

Reeves: Okay, and you start to salivate. Well, mosquitoes need a blood source, and they have to find a host. They don't find a host by going downwind searching for it. They have to go up to the source

of the odor, and in this case it's largely carbon dioxide that they're detecting.

Hughes: I would think that a mosquito couldn't fight a headwind.

Reeves: Yes, but you're thinking of a headwind that's up in your face. Now, when that's happening, they sit tight. They're not up flying around. They sit tight when there's a lot of wind blowing. But most of the time there isn't any wind or it's a mild one. Then the other thing is that they don't go right up in the wind and get their nose into it and try to fly. They get down between objects where there's not that much stress on them. When the wind's really blowing, your ankles usually aren't feeling it nearly as much as your face is.

We make our own guesses as to why mosquitoes are doing things; they won't tell us anything. I've tried asking mosquitoes questions. They never answer a question. I even think sometimes they lie to me. You have to find some way to get that information. It's like the veterinarian's problem. A veterinarian can't go up to a horse or a dog and say, "Do you have a headache?" because they just won't tell him if they have a headache. You have to find some other way to determine if they're sick. If we want information on mosquitoes--where they've been, what they ate, how old they are, all these things--we have to go about it in a tricky fashion. I don't expect a lot of cooperation from them.

### Building on Previous Research

Hughes: One of the many things you were doing in the studies was developing methodology. Did you establish criteria for identifying arthropods as vectors, or had Meyer already set up criteria?

Reeves: No, he'd really done nothing in that regard. He had found out in his plague work that certain fleas were very host specific. In other words, the flea you find on a cat and the flea you find on a mouse might be quite different species. In his plague work, he had done a lot of work to show that certain species of fleas were more important as plague vectors than others because of their host specificity. He'd also shown that different fleas varied in their capacity to be a vector on the basis of whether the organism would multiply in them effectively and whether the flea would retain the bacteria and would regurgitate them when it fed, and that there were physical structures in their pharynx that retained the



parasite. So in plague there certainly was a lot of work of this general type going on.

Hughes: But could it be generalized? Was it generalized?

Reeves: You always borrow from these things. At the same time, there was a lot of work going on with *Aedes aegypti* and yellow fever. We weren't working in a vacuum in the sense of what relationships were going to be found between certain species of mosquitoes and certain animal hosts or viruses.

What was different in our project was that we were dealing much more with what you might call a hidden disease, in the sense that we could not use infection detectable pathologically in the important animal host like you can with plague or in the human host, like happened with a high frequency with yellow fever. We were dealing with almost completely inapparent infection in the wild birds, and we didn't know anything at this stage about the relationship of different mosquitoes to different animal hosts or the viruses.

In the case of plague, it becomes obvious. If you collected a mouse, you chloroformed it, and you took a comb and went over it to obtain fleas; or you looked for ticks on it if Rocky Mountain spotted fever was your interest. You can do the same thing with a rabbit, you can do it with a dog; you can see what parasites are associated directly with that animal. You can't do that with mosquitoes and birds. You can catch the bird, but there are no mosquitoes on it. You can't ask the bird what mosquitoes have had contact with it. You can't ask the mosquitoes. So we were dealing with a situation where we knew nothing about the biological relationships of these various vectors and their various hosts. That really was the puzzle we were untangling.

We knew nothing about the relationship of these viruses when they infected the multitude of hosts that were in the environment. In other words, we didn't know if there was any difference between how much virus would be in the blood of different species of birds or between a bird and a rabbit. We didn't know any of those things at the start, and that was the focus of our major research.

Hughes: Nobody in any other field was doing that kind of work?

Reeves: Not to that extent. The closest to it was the yellow fever research in the tropics.

Hughes: How were you keeping up with work in other fields?

Reeves: In the journals and from personal contacts. I didn't have any direct contact with the people who were working on yellow fever at the beginning. I could read the journals about what they were doing, and I did. I would get the Rockefeller Foundation annual reports, which would summarize everything they were doing. They were available in the library.

### Identifying Vertebrate Hosts

Reeves: I could find out from talking to Dr. Meyer and to his group of zoologists and entomologists at the Hooper about what they were doing about these things, but they were doing it in a completely different sort of an arena and with different methods. They had the advantage; the mammals they were working with stayed locally in relatively little areas. Their parasites stayed with them. I was dealing with the free world out there; some of our birds were even flying from here to Mexico or South America and back to Kern County.

And then as we got into the problem with a professional ornithologist working with us, we found out that bird populations were very complex. We had over one hundred species in an area like Kern County. Some of those birds were there all year round; some of them were only there in the wintertime; some of them were only there in the summertime. Some of them nested there; others nested thousands of miles away. Some had their young there; some never were there during the breeding season. We found that the different species of birds varied fantastically in numbers. Some were extremely common, some were extremely rare. You quickly discarded studies on the rare ones. If an animal wasn't common in an environment and wasn't there at the time of year when your viruses were active--and we could pinpoint that; it was in the summertime--if they were only there in the wintertime, we believed they were not important to us. At least at this stage we thought they weren't. We changed our mind some about that later.

So the bird had to be abundant, it had to be there at the right time of year, and it had to have contact with the mosquito that was the carrier, which was *Culex tarsalis*.

Hughes: Now you're getting into some of the criteria for the identification of a vertebrate host, aren't you?

Reeves: That's right.

Hughes: You and your team were working out the criteria?

Reeves: We were doing it. No one else was doing it for us. That doesn't mean we didn't use knowledge from other people's work on plague or yellow fever. But they weren't working on the same sort of problem. I'm not saying ours was more complex; it was just new; it was different in many ways.

When we started inoculating these animals with a virus to see what happened, we found to our amazement that they were not all the same. It was stupid to ever think they would be the same, but until you've looked at it, you don't know.

We started the work in the forties. There was a war on, and there was no question there was a war on. This affected our human population and what it did. If you went into any of these environments in the Yakima Valley or in Kern County, at almost every house you'd find a chicken flock in its back yard, to get eggs, to have chickens to eat. There weren't that many big chicken hatcheries and farms, and meat was rising in price, and eggs, maybe. I don't know whether eggs were rising or not, but if people wanted these things, they grew their own. They had their home gardens, their Victory garden. So you had a home environment that ecologically was very impressive.

I'd go to where a person had encephalitis, and I'd interview that case. I'd go into their yard looking around, and I'd find *Culex tarsalis* there, and I'd find the darn chicken flock sitting out there. I'd bleed them, and a lot of them had antibodies to western equine or St. Louis viruses. They were being infected. I'd inoculate chickens, and they'd develop a beautiful viremia, a large amount of virus in the blood. I'd feed *Culex tarsalis* on them, and they'd all get infected.

Well, now, I'm not stupid. It was obvious that chickens were extremely important. They were the source of that human infection.

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Reeves: After the war I would look for chickens, and suddenly chickens didn't seem so important to me, because the virus was still there, the mosquito was still there, but chickens usually were not there anymore. When I wanted to bleed chickens, I went around the neighborhoods and looked for chickens, and nobody had them anymore.

Hughes: And yet people were still getting encephalitis?



Reeves: Yes. There was still plenty of virus there; people still were getting encephalitis, and the mosquitoes still were there. You looked around, and you said, "Hey, there's something wrong here," eliminated one species. It doesn't mean chickens are not important. If they're there, they're important. All those bird species that aren't there at the right time of year to transmit virus, you mentally get rid of them.

### Wild Birds as Virus Hosts

Reeves: You find then that in the summer you have the house finches, house sparrows, blackbirds, and mourning doves in large numbers. You have what we call the dicky birds in the yards and fields. There are not the great big turkey vultures, there are not the sparrow hawks, there are not the eagles. There are these common birds that are in every yard and field. Then you start looking at them, and to your amazement you find out that some of these birds are excellent hosts for the viruses and important blood meals for the vectors. When you infect them they circulate a lot of virus in their blood. They infect mosquitoes easily and for a period of several days.

Some other birds which were abundant and looked like they ought to be just as good sources didn't develop a viremia that would infect any mosquito vectors. They may have antibodies because they get infected by mosquito bites. They may have malaria infection because of mosquito bites. But they're not important as a source of encephalitis viruses because they don't get enough virus in their blood to infect a mosquito.

Hughes: Is that just a peculiarity of the beast, so to speak?

Reeves: A "characteristic" is better than "peculiarity," because there's nothing peculiar about it; it's a difference. And we don't know why.

Hughes: But it's some biological incompatibility?

Reeves: It's a lack of compatibility. There's something missing in those birds, and we don't know what it is. There's some reason why the virus cannot infect cells effectively, multiply effectively, or the birds are too reactive immunologically, so they get an immediate response which knocks down the virus, and they won't infect mosquitoes. It takes a lot of virus to infect a mosquito in most cases. If you don't have a lot of virus, your mosquito may get infected but never be able to transmit. It gets us into a

whole different game, so we have to work both experimentally in the laboratory and by observation in the field to substantiate how these things fit together. That's what makes up the field epidemiology or natural history of the infection.

Hughes: Were you and your team the first to focus on wild birds as vital to the infection cycle?

Reeves: I think so. There was a lot of work done in the East with pheasants, because pheasants were very susceptible to eastern equine encephalitis, and it was killing pheasants. A gentleman by the name of F. R. Beaudette, who worked in New Jersey, was very concerned about pheasants and their importance as a source for mosquito infection and transmission of infection to horses and humans. It was so obvious that pheasants were being infected, because they were dying from it. They had pheasant game farms where pheasants were raised for hunting purposes, and they'd have an epidemic, and eastern equine encephalitis virus would just knock them off. We looked at our pheasant farms here in California. We found some western equine virus antibodies in them but no deaths.

The only group that came into this deal very soon and very strongly was the CDC group in Montgomery, Alabama, in the early 1950s. That unit included Roy Chamberlain and Danny Sudia, who also latched onto the bird thing very strongly and very effectively in their work. But they followed us.

Hughes: Not the Rockefeller labs?

Reeves: They never had worked with encephalitis viruses until they found they were occurring in Latin America, Africa, and Asia many years later.

Hughes: I'm thinking now of this bird infection business.

Reeves: No, the Rockefeller Foundation developed their international research centers in Trinidad, India, South America, and Africa in the 1950s and 1960s, but that was much later.<sup>1</sup> I believe their first field station was established in Trinidad in the mid-fifties, where Wilbur Downs was in charge, and he and Tommy Aitken

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<sup>1</sup>For more on the Rockefeller Foundation's arbovirus research network and particularly the Poona laboratory, see: Harald Norlin Johnson, Virologist and Naturalist in the Rockefeller Foundation and the California Department of Public Health, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1991.

were the first persons that they had in the field who were knowledgeable enough and acute enough to recognize the importance of birds as hosts of viruses.

**Harald N. Johnson**

Hughes: Dr. Johnson began an arbovirus laboratory in Poona, India.

Reeves: But you see, that was later, in the fifties.

Hughes: So Trinidad was earlier?

Reeves: Yes, I believe that Trinidad preceded Poona. Well, they were about the same time.

Hughes: Dr. Johnson was out in the field trapping birds?

Reeves: Dr. Johnson was on his way to Poona, and he stopped here in the late 1940s to visit us and to review our project before he left. He was going into a completely new field as far as he was concerned. He'd been working on rabies and malaria before and during the war period, and afterwards he had a paralytic disease that kept him out of business<sup>1</sup> for a significant period.

Dr. Johnson was going to come here to visit me in Bakersfield, but he couldn't do that because I was down in San Diego and Tijuana attending a meeting of the U.S.-Mexican Border Public Health Association. So he came down there, and we spent a couple of days together. I remember we spent more time talking to each other about what we were doing and how we were going to do it and why than we did listening to papers. How much influence that had on what he did when he got to Poona, I don't know.

As you know, Harald Johnson is an unusual person in the sense of being interested in nature and field biology as well as its relationships to diseases. You can hardly make him think about anything else. So it was a very natural way for him to go. He knew in detail what had been done on the jungle cycle of yellow fever with the monkeys as hosts. The yellow fever work on monkeys never really got down to the intensely biological type of thing that we were able to do with birds--bringing hundreds of birds into the laboratory and infecting them and studying the whole

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<sup>1</sup>See Dr. Johnson's oral history.



population and its virus relationships and ecology, either in captivity or in nature.

There's a lot of difference between studying in detail the biology of the mosquito in the comparatively simple ecological unit in Bakersfield and going into a rain forest to do such studies. Dr. Marston Bates' was the one person who did this, and I was in touch with him. It's characteristic of the tropics that there is extensive speciation, but usually small numbers of each species, whereas in a temperate area you get few species but large populations. That is a real advantage for the sort of research we were doing, because we had large numbers we could work with and they weren't hidden in a dense jungle.

Hughes: You also had the advantage of known species. I know one of the things that Dr. Johnson had to do was to learn what kinds of birds were in India.

#### The Disinterest of Berkeley Zoologists

Reeves: Well, no one else had ever done a study on birds with the intensity that we did. I went to the vertebrate museum here on the campus and tried to convince Dr. Alden Miller and his associates who were here how interesting and exciting it would be to send a student to work in Kern County. They weren't interested in studying a multitude of species; they wanted a student to study some aspect of a species in detail, as this was their usual approach. It was sort of like the difference between basic and applied virology. Our research wasn't basic enough to be of that much interest to them. As a matter of fact, they weren't even interested in our publications.

Hughes: So they never participated in your program?

Reeves: Never participated in our program. I knew these people well, and I had good campus contacts with them. I'd taken their courses in zoology and ecology when I was a student, so I knew Dr. Seth Benson, I knew Dr. Miller, I knew Dr. Frank Pitelka very well. Pitelka's studies on lemmings in Alaska were of a lot of interest to me, more so than his interest in what we were doing in Kern County. Even when we started working on rodents in Kern and Butte counties, it wasn't a pure enough environmental sort of research

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<sup>1</sup>M. Bates, The Natural History of Mosquitoes, New York: MacMillan Co., 1963.

to arouse their ecological interests. That's not a criticism. It's a matter of different people having different areas of interest.

As a matter of fact, when I tried to get Dr. Miller interested in some of our publications on animal populations, he wouldn't even support their publication in the ornithological journals. That's why a lot of our data had to be published in our first monograph.<sup>1</sup> Most of the data weren't published anywhere else.

Hughes: Because it wasn't narrow enough?

Reeves: I believe in their view it was too narrow and applied. It wasn't designed as a basic ecological study of the type they wanted for their purpose. That wasn't a criticism of what we did, because we did it the only way we could learn what we needed to know.

Hughes: Did you use the Berkeley zoologists as a reference source?

Reeves: Very rarely.

Hughes: Because you knew it probably better?

Reeves: Well, they didn't have any reference that would help us. Identification of birds was only a problem on occasion. I'll give you an example that's a very simple one. In the late fifties and early sixties, when we started studying the overwintering of these viruses, one of our problems was how the viruses overwinter. Do they overwinter in the mosquitoes, or do they overwinter in the bird hosts?

#### Virus Sampling of Birds and Mammals

Reeves: So in Kern County we began to intensively sample the mosquitoes to see if they were still infected during the wintertime. We also went out and intensively sampled the birds and the mammals that were in these areas in the wintertime at places where we knew the virus had been active the previous summer. We collected every species of bird and every species of mammal that we could find in

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<sup>1</sup>W. C. Reeves, W. McD. Hammon. "Epidemiology of the Arthropod-borne Viral Encephalitides in Kern County, California, 1943-1952," University of California Publications in Public Health, Volume 4, University of California Press, Berkeley, 1962.

the Buttonwillow area, which is out on the west side of Kern County where the virus had been very active for years. By sampling I mean we collected those animals and brought them in, anesthetized them, bled them, and we took eight organ samples for virus testing from every bird, every mammal.

Hughes: Is that a scheme that you worked out?

Reeves: Yes. We took the blood, we took the spleen, liver, lungs, brain, kidneys, lymph nodes; we took everything except the squeak or chirp. Those samples were tested intensively by the best methods we had to see if there was any virus in them, and we made a few virus isolations in the middle of the winter, December and January. I mean really in the middle of the winter.

Hughes: And the virus was not supposed to be there?

Reeves: Well, it had to be there, but it wasn't necessarily supposed to be where we found it. It has to be there, because otherwise how are viruses going to be there every summer unless they are reintroduced?

Hughes: That was the question, wasn't it?

Reeves: That was one of the basic questions. But here it is; it's there, in these animals. But what's it in? A white-crowned sparrow, a house mouse, and an antelope ground squirrel--three species of animals.

Now, you've asked me if we needed to get help on identification. We never had found a white-crowned sparrow that had any antibodies in it. If there are no antibodies in them, we assumed they had not been infected. Number two, this bird never was there in the summertime. The species was strictly a winter resident in Kern County. Now, we knew that because McClure had been out counting birds sitting on fence posts and seeing what side of the limb they hung their tail over and all those things. He knew all about those birds. And they were not there in the summertime. As a matter of fact, if we caught some of those birds and put them in cages, when it got hot in the summer they just died. They couldn't stand hot weather.

We went through the literature and found out that the white-crowned sparrow was not a simple bird; there were several subspecies. My memory may be a little bit off on this, but one of the subspecies spent its time in the summertime in the mountains in the Sierras, one spent its time up in Oregon, Washington, nesting there, one went way up into Alaska, and so on. That may be a little bit off, but it's that varied. We didn't know how to



tell one of these subspecies from another; they looked the same to us. But we found out that there was a zoologist-ornithologist, Dr. R. C. Banks, who was very interested in this complex and who was interested in collaborating with us.

Anyway, we found that the subspecies which predominated in Kern County spends its summer in, as I recall, British Columbia. There's no western equine virus active in British Columbia that we know of. Here's western virus in a bird from up there, and there were no antibodies in hundreds of specimens of that bird when we collect it in Kern County. Well, that creates some real problems, doesn't it? Because we knew the birds that we were interested in were the house finches, house sparrows, blackbirds, et cetera, and many of them had antibodies, but we found no virus in them in the winter.

The first question was, why no antibodies? That was simple. We took the virus we'd isolated from white-crowned sparrows, we caught some birds and inoculated them, and bingo! All the sparrows were dead within forty-eight to seventy-two hours. It's a highly lethal agent in them. Now, if a bird's infected and dies, it's not going to develop antibodies. We had to do hundreds of birds before we got some survivors who showed us that if they got the infection and survived, they would develop antibodies. So what that meant was, literally, we had a dead-end host in our hands.

So we looked around; now, where else could we get virus? We got it from a lousy house mouse. We got it from an antelope ground squirrel, another small rodent that's out in the desert. We'd never found antibodies in those species either, and we'd never identified a blood meal from a mosquito that had fed on any of them. But the virus was there, and to this day I can't explain it, except that someplace in that environment they must have been getting infected at that time.

### Virus Overwintering

Reeves: At the same time, in the same area, we never got virus out of the mosquitoes in December. We found no virus between mid-November and mid-January, and we'd collected thousands of the damn things. Excuse me. Erase me. I didn't really mean that. [laughter] So from mid-November to mid-January we never saw a virus in a mosquito.

Hughes: But you don't have to have it there for it to overwinter?

Reeves: It's got to be there somewhere.

Hughes: Eventually, but why over the winter?

Reeves: Because that's the critical time frame. In the summertime we have no difficulty accounting for the virus and the series of transmissions that takes place. But where does it come from?

Hughes: Yes, but when they're sitting there in the winter--

Reeves: Sitting where? We're trying to find out where. What are our possibilities? The first thing we think of is that it ought to be in the same place (that is, vectors and hosts) that it is in the summer. That makes sense to me. If it's there in the winter, it ought to be in the same place it is in the summer; it shouldn't be someplace in the winter that it isn't in the summer. Otherwise, how does all this happen? The other alternative is that it has to be brought in.

Hughes: How could you rule that out?

Reeves: It was there every darn year at the same sites over a large region. It's there every year, just like that. Bingo, we could tell you the week, the day, the hour it was going to appear in the birds and the mosquitoes. Every year, May to June, and it's there through October.

When we started testing mosquitoes in the winter we found western and St. Louis virus in the vector until we'd got up to the end of October, the first of November, the middle of November. All of a sudden, no more virus. We could find mosquitoes; it wasn't easy, but we still collected them between November 15 and January 15, five thousand female *Culex tarsalis*. That's not a small sample, because you can go out and spend a whole day and get maybe five females and you've done a good job. So there are not very many going through the winter.

What happened was, no virus from the middle of November until the middle of January--none. From the middle of January on, the viruses showed up in those mosquitoes. It was there in January, February, March, April, May, and June. It built up in the summertime, but it was there earlier. You say, the mosquitoes are carrying it through the winter, then. That's simple enough. The mosquitoes are infected and are carrying the virus through, but why can't we find it in that two-month interval?

Hughes: What about the sensitivity of your tests?

Reeves: The best tests we have.

Hughes: But are the best good enough?

Reeves: How do you answer that? That's one of the questions: is the best good enough? All our efforts to develop the most sensitive and specific tests have so far not improved much on the methods that we were using. We have used tissue cultures, plaque assays, DNA probes, immunoassays, serial blood passages, et cetera, et cetera. None have been as good or much better than what we did earlier.

But we did find out in studying *Culex tarsalis* that those mosquitoes stopped taking blood meals in October-November, and the population that survived from that time period until January was an overwintering population in what we called diapause, suspended animation. Most of them had not taken a blood meal, and they'd never laid eggs. That was a virgin population of mosquitoes. It was not virgin really; they were inseminated. But they were clean; they hadn't taken blood. In the middle of January, which is the coldest time of year, their fat reserves were depleted, and they had to feed on blood. They came out, and 100 percent of that population fed in a couple of weeks, in the middle of the winter. So there was obviously virus there that they were picking up.

We could infect mosquitoes artificially in the fall and put them into a cellar at outdoor temperatures, and some would survive if we nursed them long enough, and we could carry virus through the winter in them artificially. But that wasn't what was happening in nature. That still is one of our big dilemmas.

### Carbon Dioxide Release by Birds

Reeves: I was interested in how much carbon dioxide I would have to release for mosquito attraction. I wanted to duplicate the amount a house sparrow or a house finch would put out. How much would a person put out? How much would a cow put out? This seemed simple enough; I mean, somebody's got to know this. I went over to the vertebrate museum, and I went to Benson, Miller, and Pitelka. I said, "Hey, how much carbon dioxide does a house sparrow give off?" That was the dumbest question they'd ever heard. They couldn't care less how much carbon dioxide a bird exhaled.

Finally I found another fellow at the museum, Dr. O. Pearson. He was interested in hummingbirds, and he'd found out how much carbon dioxide a hummingbird gave off, and it wasn't much. That wasn't getting me very far, because hummingbirds were of no



concern to us. But anyway, to make a long story short, we finally worked out that chickens would give out about twenty-five milliliters of carbon dioxide per minute.

Hughes: Did you work that out?

Reeves: I went to the physiology department. That's a pretty logical thing, to go and talk to them. I went to Dr. [Shelburne] Cook, the head of physiology at that time. I'd taken a physiology course from him.

Anyway, I went to him, and he was the professor of physiology, and he knew everything. I said to him, "I need to know how much carbon dioxide a cow gives off." He said, "Have you looked in the literature?" I said, "Yes, I've looked in the literature." All he said was, "Any physiology book tells you how much a person gives off, how much a cow gives off. Everybody knows that." I said, "That's fine. How much is it?" [laughter] He said, "Go look it up in the library." I said, "I've looked in the library. I can't find it." He said, "That's dumb." That was a typical reaction he had; and I was a former student, so I was dumb.

So he reaches up and pulls the first physiology book down, and he looks; it's not there. He picks another book out, and it's not there. Finally he called in his flunkie, his graduate student, and he said, "How much carbon dioxide is given off by a person or a cow?" The student said, "I don't know." He said, "You go look it up and come back and tell me."

This was back in the fifties, and there had been enough basic metabolism work going on that 250 milliliters per minute was a good guess for a human being lying down flat on his back, and 2,500 milliliters for a cow, because one cow in Wisconsin had been put into a physiological chamber and everything measured going in and coming out, including carbon dioxide. That's what you get by going to the experts. Sometimes you're lucky, sometimes you're not.

We studied releases of 25, 250, and 2500 milliliters of carbon dioxide in mosquito traps as a simulation of the amount of carbon dioxide given off respectively by a chicken, man, and horse or cow. The species of mosquitoes collected correlated with knowledge of the host preference of the different mosquitoes.

Reeves' Rules for Vertebrate Reservoir Hosts

Hughes: What we've been talking about is what Dr. Karl Johnson calls "Reeves' Rules for a Vertebrate Reservoir."<sup>1</sup>

Reeves: Yes.

Hughes: Of which there were five? Am I getting them mixed up?

Reeves: I don't think you're mixed up, but again, I don't carry these around on the top of my head. I've referred to most of them. The host has to be common in the environment. It shouldn't pass on antibodies effectively to its offspring. It ought to be characterized by having a high titer of the organism in the blood. It shouldn't confer a fatal disease, because that would decrease the host population. And it should be a preferred host of the vector. So basically we've talked about all those requirements.

Hughes: Were these listed in pretty much this form in your thesis?

Reeves: Yes, but it wasn't all my thinking. Everybody wants to give me all the credit for it, but I'm sure it's the outcome of a lot of conversations I had with a lot of people, a lot of work that was done by a lot of different people, and some reading. The thesis is one of those times when you just sort of sit back and think about a lot of these things and put them down. The same thing with the characteristics of a good vector; I also outlined those in the thesis. Sometimes you find you have done nothing more than reinvent the wheel, forgetting where the idea came from.

Hughes: Were they pretty much adopted as such by people in your field and related fields?

Reeves: I guess so. They get referred to in publications periodically. Not very many people have read my thesis. My thesis was never published as such. There's a copy over here on my shelf. Dr. Hardy bought the one copy that was on file in the Biology Library on campus. That's the first time I knew the Biology Library would sell a thesis to a person. He wanted it, so he got it. In fact, he rescued it, and he has it on his desk. So now there are two copies of that thesis available in the world that I know of. Unless Karl Johnson made a copy when he had it, because he borrowed my copy, and he may have made a copy. I don't think

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<sup>1</sup>K. M. Johnson, Professor William C. Reeves: Scholar, Teacher, and Friend, American Journal of Tropical Medicine and Hygiene 1987; 37: 35-75.

he did, because Karl doesn't carry much paper around with him, he moves so frequently.

So if Karl doesn't have one, there are two copies. Now, who in the heck is going to refer to my thesis if they can't get their hands on it? The so-called "Rules" were picked up and duplicated in part by quotes in the paper that Karl Johnson wrote for the symposium on my retirement, so they are there again. They are in some of our publications, so it's in publication, but not with just me as the sole author. Credit for something like this is not a priority as far as I'm concerned, because the main thing is that it becomes a part of general knowledge.

It's like all research. You make a discovery, and for a few days, a few weeks, maybe a month, at most a year, it's very exciting, it's very new. Then it becomes part of everybody's knowledge within that field, or it becomes a part of public knowledge, if it gets enough publicity in the lay press. It's no secret to most people who live in the Central Valley of California that mosquitoes carry a virus out there called encephalitis. At least it didn't used to be a secret. I think it might be news to today's population, because it hasn't had a lot of publicity lately. But we thought that was a great discovery when we made it. It was good enough to make Science, which everybody holds as some sort of a standard, although a bunch of junk shows up in Science, too. But it's not news very long, and it's not just an individual credit sort of thing. A main reason you do research is because you get some degree of personal satisfaction and want to get it into the general knowledge of the field so that it gets utilized. At least that's the way I have looked at it.

Karl Johnson made a big deal out of the criteria for a vertebrate host and a graduate student having thought of them, but to me it didn't represent only my synthesis. Dr. Hammon is credited generally for the term "mosquito-borne encephalitis," which became arthropod-borne virus encephalitides, which finally became arboviruses. Well, he didn't know an arthropod from a hole in the ground until I told him about them. And that's not being derogatory; I mean, it's just a matter that it wasn't a part of his science lore. He knew the virology of the time, and I knew entomology. But the main thing is that this epidemiological or biological type of an association became set in a lot of minds. There was this group of arthropod-borne diseases that was worth thinking of in that sort of a context as far as prevention was concerned, as far as understanding how the disease was transmitted and how people got it. I feel the same way about the criteria for an effective vector or an effective vertebrate host. Actually, at the time you organize those ideas and put them together, you don't have any great sense of "Gee, guess what I just thought of."



You're just trying to get your own thoughts into an organized format. At least that's what I tried to do.

More on William McDowell Hammon

Hughes: Can you make any generalization about what sphere of knowledge was under Dr. Hammon's purview and what was under yours?

Reeves: For his day, for practical purposes he was as well trained as a person could be in virology as an emerging field. As I said earlier, my knowledge of virology when I came into this was based entirely on what was touched on in passing in a microbiology course which was primarily bacteriology. Things were called viruses that aren't even known as viruses now; they're something else--a bacterium, a rickettsia, a prion, or something. The field was very undeveloped. He had come from a laboratory that was doing virological work on polio. He didn't even work on arboviruses when he was a student, but he had good basic training in microbiology so he really knew the theory and the application of the neutralization test and the complement fixation test that hadn't been a basic part of my training.

So I had to learn those things from him at the lab bench, and that's indeed what happened. We went out in the field in the summertime and collected an awful lot of mosquitoes to be tested for virus, or animal blood to be tested for antibodies. I'd come back in the wintertime to the lab, and a lot of times specimens hadn't been tested, or something had happened when they were working on them, or nobody was in the laboratory except Hammon. He was busy doing a lot of other things, so I had to sit down with him and get a very intensive training in what the methods were, enough to at least do them. If something went wrong with the test, I had to run it again to find out what had gone wrong, whether I'd screwed up someplace or we had to modify the test or something.

So virology he knew. He knew a lot more pathology than I did. Pathology wasn't that critical, and we always had Dr. Meyer as our principal crutch if any pathology questions came up, because he was a top-notch, number-one pathologist. Dr. Hammon knew medicine. I didn't know medicine, and he was well qualified as a physician.

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Reeves: Now, to give you an example of his degree of knowledge compared with other people: When we were working in Kern County and ran into the problem of differentiating polio, St. Louis, and western encephalitis, he realized he had a real problem. He couldn't separate these diseases clinically in all cases. The National Foundation for Infantile Paralysis, our great white father who was financing the project, said, "That's the trouble with people from the West; you're not very sophisticated in this regard."

Hughes: That sat well.

Reeves: Yes, that sat well. They forgot that Hammon was trained at Harvard.

But anyway, as I said earlier, they sent one of the top neurologists from Harvard Medical School all the way out to Bakersfield to show Hammon how to differentiate these infections clinically. The guy fell into a real trap, because he didn't know that we'd finished the lab work on some of the cases that were still convalescing or coming back for therapy in the hospital. We knew some were St. Louis encephalitis and looked just like a typical polio case. The visitor came in, and he said, "This is a typical polio case, and this is a typical encephalitis case," and so on. After he'd done all this, Hammon said, "Would you like to see the lab records on these cases?" He said, "Yes, I'd like to see them," and there he was, cold turkey wrong. He thanked us profusely for the visit because--

Hughes: --he learned a lot. [laughter]

Reeves: --he learned a lot, by coming to the West.

To get back to Hammon, he was a good clinician. For instance, when we'd go out in the field and he'd be called into a hospital for a consultation, he knew exactly what material he needed from the cases. If there was an autopsy, he knew exactly what material he needed. If he needed to do a spinal tap or anything else on one of these people, he was quite competent to do it. He'd done a lot of them before he was even a physician in Africa because of African sleeping sickness, trypanosomiasis. So basically he was a good physician; he was well trained, he was respected at the medical center [UC San Francisco] for his clinical knowledge. So he knew medicine, he knew pathology enough that he'd be able to handle that end of things, he knew virology.

I knew none of those areas, except what I'd picked up on occasion. I had taken courses in the university which amounted to all the pre-med requirements and even more than that. I'd taken a course in pathology and a course in vertebrate anatomy and these

sorts of things, but that didn't fill that gap. And it wasn't my business, plus it was his and he was licensed.

The differentiation of these diseases was extremely important as well as working out their natural history. Now, he was no dummy as far as natural history was concerned. When he went to Yakima the summer before I had any contact with him, he knew enough to begin to collect samples of blood from different domestic animals. He didn't collect any wild animal bloods, because at that time we thought this was a horse disease and also in domestic mammals and man, so he collected those bloods and found antibodies in them.

So Hammon established the diagnostic facilities and the diagnostic tests, got the clinical histories of these people, opened the door to the other areas, and then recognized that he needed competence in these other areas. I was fortunate enough to be the guy who was standing in the wings and got called on stage. It was purely coincidence. But he knew he needed people with my sort of a background and training.

#### Educating Physicians about Encephalitis Diagnoses

Hughes: Was it also important to work with the physicians in the community?

Reeves: You had to. You can't touch a case without working with the physician whose case that is. You have to realize that when somebody is sick and goes to a doctor, they become his or her case. You don't talk to that patient, you don't take a specimen from that patient and survive in that community unless you have the doctor's agreement that it ought to be done and they're responsible. It's their case.

Hughes: But that required educating the local physicians about encephalitis, did it not?

Reeves: Yes, but Dr. Hammon was meeting with the medical societies. In every one of these counties they'd meet him. I'd also go with him to those medical society meetings. We were educating them as fast as we could. Anytime we went on a ward of one of the hospitals, if there was an intern or a resident there, we educated them.

Hughes: But that's a nonstop proposition, because those people don't stick around, and you have to educate their replacements.



Reeves: They rotate.

We had this epidemic of St. Louis encephalitis in Kern County in 1989. When I had left California late in the summer on a trip, there was no evidence of any virus activity in the Central Valley. I went off to Colorado fishing, very happily, no problem. I also went up to Canada to a meeting. When I came back, the reports of the state surveillance system from the state laboratory were on my desk; they'd accumulated during the month I'd been gone. As I started to read I suddenly sat up straight, as practically every mosquito pool from Kern County was testing positive for St. Louis virus. The sentinel chickens were all getting St. Louis infection. So St. Louis virus for the first time in a number of years was active again in Kern County, and I just sort of sat here looking at the reports dumbfounded.

I called Dr. Richard Emmons at the state lab and said, "Dick, this is pretty exciting, all these viruses from mosquitoes and antibodies in chickens. What's happened to human cases?" He said, "What cases? I haven't gotten any samples."

To make a long story short, the virus was there, and it was known. I didn't know, because I was on vacation, but the mosquito abatement district knew about it, and they were out there frantically trying to kill mosquitoes. I called the manager of the district down there and said, "Harm,"--his name's Harmon L. Clement--"where are your cases?" He said, "What cases?" I said, "You can't have all this virus activity and no cases. What's the health department doing?" He said, "I don't know." I said, "Then why don't you go ask them. Find out what's going on at the hospital and the health department." He said, "Do you think I ought to?" I said, "If there's going to be all this virus activity, and if there are cases, you ought to know about them. You are out spraying mosquitoes wholesale to prevent cases; you better find out what's going on with disease."

It turned out that they were having the biggest outbreak of so-called aseptic meningitis they'd had for years in Kern County. What does aseptic meningitis mean if you remove the jargon? It means there's something affecting the meninges of the brain which is not caused by something you can culture, like a bacterium or fungus. It's just that simple. Which means the term is a clinical wastepaper basket.

To make a long story short, when we got a preventive medicine resident (Dr. John Tueller) out there looking for cases in Kings, Kern, and Tulare counties, we had twenty-eight cases of St. Louis encephalitis that had to be proven in retrospect. They'd already happened. Well, why did this happen? We had written the book on

encephalitis in Kern County.<sup>1</sup> So how can it be? I mean, you would think everybody down there ought to know about those diseases. The problem was complex. A new health officer, trained in Africa, replaced the former health officer, who had been in the army and knew about encephalitis, but this new person had never been in Kern County before. The assistant health officer was from India, and her knowledge of this disease was scanty. They knew they had an epidemic of aseptic meningitis. There wasn't a doctor left in Kern General Hospital or other local hospitals who had ever seen a proven case of encephalitis. They were all gone. The residents who were there earlier for one summer and had seen the cases were long gone. The copies of our earlier ten-year monograph were no longer in the hospital or health department libraries. The only copy was at the mosquito abatement district. The disease had not been diagnosed. Why?

It used to be that every summer we'd go down there, and we'd take the medical students over to the hospital and have them follow up on cases. We'd talk to the whole resident physician population, the nurses, laboratory staff, and everybody to reorient them. The new ones were oriented. After a couple of years I had realized I'd better go back over there in July because there was a whole new group of residents, and they had to be oriented. By the next year they were gone. You had to meet with the medical society anytime you got an opportunity to tell them about encephalitis and encourage them not to just let the cases come and go but to submit diagnostic samples!

One problem was that if you got a diagnosis, it didn't help the physician any on treatment. If it was western or St. Louis, there was nothing they could do except give the best care. If it was T.B. or coccidioidomycosis meningitis or at least a bacterial disease, then they could do something with antibiotics, but if it wasn't, just good common-sense medicine and hospital care was all they could offer.

Hughes: So for practical purposes, physicians didn't really care whether it was western or St. Louis?

Reeves: To a degree, that's right.

Hughes: Did they take the trouble to try to differentiate?

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<sup>1</sup>W. C. Reeves and W. McD. Hammon, "Epidemiology of the Arthropod-borne Viral Encephalitides in Kern County, California, 1943-1954." University of California Publications in Public Health, Volume 4, University of California Press, Berkeley, 1962.



Reeves: They would be very supportive if they knew there was someone really interested and who was going to give them back the answer.

Hughes: You can't use the age of the patient as a clue?

Reeves: No. Because every age gets encephalitis. Now, it's true that infants under one year of age are the most susceptible to western, and people over 60-65 years of age are the most susceptible clinically to St. Louis, but there are cases through all the age groups. They are not as common in some, though.

Hughes: Wasn't this a matter, too, of instructing in some of the basic techniques of sample collection?

Reeves: You had to be sure you got a serum sample. If you are a case, they not only bleed you, they do a spinal tap; because when a person gets encephalitis, the pressure in the spinal fluid goes way high, and that's what gives a very severe headache. So they routinely do a spinal tap to release that pressure and also to get a sample of cells to see how many lymphocytes and how many other types of "cytes" there are in the blood or in the spinal fluid. So here's the spinal tap sample in their hands, and so they say, "Hey, we'll send this into the lab."

It happens that spinal fluid usually is not a good indicator sample for antibody, and it's not a good place to get these viruses. But a physician may not realize this. So you receive a spinal fluid sample at the lab, and you say, "Hey, this isn't the sample we need; we need blood samples." You explain, as we had to them, that we need two blood samples: an acute phase and a convalescent phase sample so we can show a rise in antibody as a diagnostic indicator. Physicians don't have too many diseases they need to take two samples on. So then they asked, "What do we do? Do we take a sample when the person leaves the hospital?" We said, "We really want the second sample to be taken two weeks after the first sample." Well, the person's gone home, and it takes a real sales job to convince them or to have somebody else, like a public health nurse, do a follow-up.

Now, it wasn't just the medical personnel who had to be contacting us but also the veterinary people, because western equine encephalitis was an important horse disease. So Hammon and I spent a lot of hours traveling up and down this state and other places in the western United States, meeting with both medical and veterinary societies at a state, county, or bi-county level. I had summers when I met with almost every veterinary association in the Central Valley California.

Hughes: How did you initiate those contacts?



Reeves: I let them know I was available and that I had information that would be of interest to them. They knew I would be a speaker who would tell them the latest.

Hughes: You sent them a letter, or you called them up?

Reeves: They got the word. If you talked to the Kern County Veterinary Association, the next thing you knew, the Tulare, Sacramento, and Redding groups were in touch with you. They've got their own networks, and so do the medical groups. They're always looking for speakers. The same is true for Rotary, Kiwanis, and other such groups if you want to get information into the community. My last talk to a Rotary Club was in New Zealand, where they found out I was in town. They wanted to talk about AIDS more than my virus, so I did what they wanted.

In Kern County I was very fortunate originally in that the Kern Veterinary Hospital was the only place there were large animal practitioners. Walter Sterns came in later and established a large animal practice in his office. But when you have a single veterinary hospital that gets all referrals of sick horses, that's easy. You're buddies; you work side by side with them. Walter Sterns and his brother established a practice which was separate from the Kern Veterinary Hospital. That became very convenient for me later, because Walter became a state Democratic senator for Kern County. He became the senior senator, and that was a very convenient contact.

Hughes: You had cause to remind him of the relationship?

Reeves: Didn't have to. Earlier when Walter was in practice he would call me at eleven o'clock or one o'clock at night and say, "Bill? Sorry to wake you up, but I've got a dead horse out here. Do you want to come and get what you want from it?" I'd say, "Sure do, Walter." I'd jump in the car and go out, and we'd chop the head of this horse open and dig out what I wanted and thank the farmer for calling us when the horse died. That's cooperation.

Hughes: But also later, when he was in the senate?

Reeves: I didn't have to remind him who I was. Once you've gone out to the rendering plant just before they take a dead horse and put it through the grinder and you do your job and work with them and send back the result, no more problem of his knowing who you are.

More on Mosquito Abatement Districts

Reeves: The same thing with the mosquito abatement district. You travel up and down the state talking to mosquito control people. In those days they didn't have their regional meetings. Now they have the San Joaquin Valley, Sacramento Valley, and Coastal and Southern California regional meetings, so they have a big meeting where they all get together for discussions or training. It's easy to go talk to them then.

Hughes: But earlier these were all discrete and not necessarily connected districts?

Reeves: The districts are completely discrete entities, politically. Each of the districts, as I said earlier, is formed by a group of taxpayers deciding that they want to have a mosquito control district, and they petition for it and it is developed. The county board of supervisors of a county--or counties if there's more than one county involved--appoint people to be the district's board of trustees. Then that board hires a manager and turns the business over to him. The district is a politically independent unit; they're not responsible to any other county agency, and they're not directly responsible to the state in any way. They're independent, like a water district or other local agency.

Hughes: The boundaries aren't necessarily contiguous to another abatement district?

Reeves: Sometimes they are, and for others not so. Back in the forties there may have been fifteen or sixteen districts in the whole state of California. But after the war they just burgeoned, and now we have sixty-three or so districts in California.

Hughes: They cover the whole state?

Reeves: No, they don't cover the whole state. They cover almost all of the Central Valley, with a few exceptions, and for practical purposes all the heavily populated areas of the state. The problem is that some of the districts are very small, so you'll have a big district almost surrounding a small district. This is a problem, because the small districts don't have the resources that the big districts do as far as money and personnel and so on.

Even in Kern County we have three different mosquito abatement districts. We have the Kern District, which is the largest one in the area; we have the West Side District around Taft, which is a large district but not as well staffed and functional as the Kern District; and we have the Delano District,

which is half in Kern County and half in Tulare County. It is another of the smaller districts. At the other extreme you have one like Sacramento-Yolo, which is bi-countywide. Orange County--countywide. So some are countywide or more, and some are not. Some districts may be just thirty square miles.

Hughes: It boils down to who is willing to fund a district, does it not?

Reeves: That's right. And then sometimes a district gets formed, and even though it's obvious that it would be more economical to amalgamate it into a larger district, it is not. For whatever reasons, whether it's political or personality or whatever, some districts don't want to amalgamate. So it's a less efficient system, and everybody knows it, but they won't yield.

As a further example, there used to be a bi-county health department in Sutter-Yuba County. As you know, Sutter and Yuba counties are not heavily populated areas. Sacramento-Yolo Health Department also covers two counties. Just recently, Sutter-Yuba became two independent health departments. It turned out that the basic laboratory and some other facilities were located in Yuba County, and Sutter County had to develop its own facilities. It's not as efficient a system, but for whatever reasons, the division happened.

In some cases health departments also take the responsibility for mosquito control. When a health department takes that responsibility, it's obviously countywide, but it usually doesn't get nearly the degree of funding that an independent mosquito abatement district would in the same area.

Hughes: Why is that?

Reeves: I'll give you an example. The Kern Mosquito Abatement District's annual budget was over \$2 million in 1990. The budget for the sanitation division of the Kern County Health Department isn't that big. The health department's environmental health program has a much wider responsibility for health matters, but they don't have the ability to put the resources into a focused program which it is really going to get as a mosquito control district. The annual budget for the Sacramento-Yolo County Mosquito Abatement District is over \$4 million of local tax money. Now, \$4 million, in a health officer's mind, is a hell of a lot of money, and he usually doesn't have that much money for his environmental health program. At the same time, if you don't have that sort of money, you cannot carry out an effective mosquito control program.

So basically, you see, there are relative values that are constantly drifting in and out in the allocation of public money



to health-related problems. I can't get accounts on budgets that are just for mosquito control in counties where health departments do the mosquito control, because they aren't published annually in the year book of the California Mosquito and Vector Control Association. The reason is that they are not dues-paying members of the association, so they are not in the yearbook.

Hughes: Now that encephalitis seems to be less of a direct threat, what is the motivation for continuing the funding for these control programs?

Reeves: The motivations are several. One is that people don't want these diseases to come back; they don't want malaria or encephalitis to become a problem again. How long that attitude will continue, I don't know.

The second thing is that mosquitoes not only carry diseases, but also they're pests. They can be very severe pests, enough to have stopped or slowed down the development for a while of the Bay Area because of the salt marsh mosquitoes. They stopped or slowed up developments in Kern County, first because of malaria and then just because of pest mosquitoes. Actually, when we came in on the encephalitis problem, the districts in that region didn't have any known reason to control mosquitoes as disease vectors. Malaria had been controlled. But mosquitoes still were such an important pest problem that people wanted mosquito control.

The other very interesting phenomenon is that if you go into an area where there's never been any mosquito control, there are mosquitoes coming out your ears; I mean, there are mosquitoes everywhere. I've gone out and talked to a farmer in such areas, and he'd be standing there talking about mosquitoes. He may say, "We don't have any mosquito problem," but he's standing there swatting mosquitoes like crazy, standing in the middle of a rice field or a cotton field.

Now, the reason is that this person doesn't react to mosquito bites, and it happens to him every day, so there's no problem. He's used to it; he grew up with it. You take somebody from town out there, and they'll run for the car. You start controlling mosquitoes in that farm area and reduce the mosquito population, and pretty soon if that farmer sees a mosquito, he'll call up the health department or call the mosquito abatement district: "We've got mosquitoes out here. What's wrong with you guys? I'm paying you all these taxes. I don't want mosquitoes biting me, my family, or my livestock."

In Contra Costa County, out by Martinez, last year a big flight of salt marsh mosquitoes came into this housing

development. The phone was ringing off the hook at the mosquito abatement district. At first the mosquito abatement didn't even know specifically where those mosquitoes had come from. They had to be salt marsh mosquitoes because that was the species causing the problem. They had to do adult mosquito spraying, because the city people who were now living out there in the country don't like this. They wanted relief from a pest. There was no encephalitis or malaria present there, only pests.

It's like the famous story of the Portuguese dairy man. He says, "Here I've had this dairy farm that's been in my family for two generations. So what do they do? They build a housing development next to my dairy, and all these tomatoes come out here in little pink shorts and bras, and the flies bother them or a mosquito bites them, and they blame me. There wasn't any problem here until they came." [laughter]

But, you know, those are the facts of life. There wasn't any problem out there. He didn't have any problem with mosquitoes or flies until the people came. They come out there dressed indecently in his mind and exposed themselves, and sure, the mosquitoes or flies were going to bother them. If they went in the house, they wouldn't bother them. The fact remains that the dairyman will be forced by the laws and by the agencies--mosquito control and health department--to clean up his dairy or move. Usually the latter is what they wind up doing.

Hughes: Even when the insect is not a health hazard but just an annoyance?

Reeves: That's right, just an annoyance, or people perceive that the insects are a health problem. This is like flies. Everybody knows they are dirty. You don't want them crawling on your food. But you try to get evidence of how many diseases are really spread by flies in one of our modern communities, and even though they may be abundant, you can't find the disease to go with the fly. Even go one step further to cockroaches; nobody wants to see a cockroach. If one ran across here right now, you'd have a fit. Why? Because cockroaches are dirty, right? And they spread diseases, right? But you can't name one disease they spread.

Hughes: That's true.

Reeves: And you can't name case number one they've caused. Man has been trying to pin the rap on them ever since he started working in this field.

Hughes: Why pick on the poor old cockroach?

Reeves: Because they don't look good, you know. They're dirty, and they smell bad.

The bedbug, the poor old bedbug. It's not fun sleeping with bedbugs. You've probably never done it. I've slept with bedbugs. It's a miserable experience, but they don't carry any disease that we can find. But nobody wants bedbugs. Spoils a lot of beautiful dreams. So basically, you see the problem there.

Hughes: Yes, I see the problem. This is switching a little bit, but it is still on bugs. [laughter]

Reeves: You're getting educated where you don't want to be educated. [laughter]

Hughes: What do you expect when you're talking to an entomologist!

Reeves: Bugs! [laughter]

#### Research on Bird Mites

Hughes: Anyway, I read a paper given by Dr. Roy Chamberlain during your retirement celebration in 1987, and he mentioned bird mites, which he suspected were hosts and/or vectors of encephalitis viruses.<sup>1</sup> Was this just another arthropod that you tested, and did you get any virus from it?

Reeves: Yes. Basically, mites were an interesting problem. The studies developed in parallel in widely separated geographical areas. In Kern County we obviously were working very closely with birds and mosquitoes. We had an ornithologist (Dr. McClure) there, and one day he comes in and says, "Boy, the bird nests out there are covered with mites." Mites are little things more closely related to spiders and ticks than to mosquitoes. They're ectoparasites on the birds, and they live in nests because that's where the birds come back each year. The mites feed on the birds' blood very rapidly and stay in the nests, wait for the birds to come back, and then feed again. They also feed on the nestling birds that stay in the nests.

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<sup>1</sup>Roy W. Chamberlain, Historical Perspectives on the Epidemiology and Ecology of Mosquito-borne virus Encephalitides in the United States, American Journal of Tropical Medicine and Hygiene 1987; 37(3) Suppl.: 85-175.



Now, the ornithologist was collecting bird nests because we were studying the birds, and he was bleeding young birds and old birds. When he put his hand into these nests, the mites came out, and he was covered with mites. They'd crawl all over him. They weren't biting him, but they were crawling on him. Then he would look at them, and they were full of bird blood. So here's the answer to all our problems. Maybe we have found a reservoir of viruses, because the birds were loaded with virus that was a source for mosquito infection. Now we had mites that lived with the bird intimately and constantly in contact, so they must be involved in virus transmission.

So we started testing mites for a virus, and we isolated western and St. Louis viruses from mites very readily. This was pretty exciting. Then I established colonies of the mites in the lab, and I could keep them and have them feed on chicks instead of wild birds. I would infect the mites by feeding them on a bird with virus in the blood, and I would test the mites after just a day or two. They originally contained the virus, not necessarily infected, but the virus was in their gut. In a few days the virus would disappear, and there was no way I could get the virus either to continue to multiply in the mite and to be transmitted by their bite.

Meanwhile, in St. Louis, Missouri, a scientist by the name of Dr. Margaret Smith at Washington University School of Medicine came into the mite picture. She was a very famous pediatrician, and [R. J.] Blattner and [F. M.] Heys, two young physicians, were working in her laboratory and had gotten interested in encephalitis, because historically this was the home of St. Louis encephalitis. They found some chicken houses that had mites in them. They started testing them, and they isolated St. Louis virus. Then these three physicians established a colony of the mites and fed them on infected chickens. They came out with a series of papers which said they could infect these mites. They claimed the mites would readily transmit virus by bite to other chickens. These mites also carried the virus through the eggs to their offspring. They had the whole answer to the problem of a reservoir for St. Louis encephalitis.

Now, they were working with the chicken and the chicken mite, and we were working with wild birds and wild bird mites, in both cases a different species. I found out what they were doing, and we went through the chicken houses of Kern County with a fine-toothed comb, and there were no chicken mites on the chickens in Kern County. There were ticks on them but no mites.

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Reeves: I went back to Yakima, Washington, and found the same species of mites on chickens that Smith, Blattner, and Heys had worked with, but there wasn't any virus in them. There also wasn't any virus left in Yakima at that stage, either. So that didn't fit together with their findings.

At a meeting of the American Society of Tropical Medicine and Hygiene I ran into Roy Chamberlain from the Centers for Disease Control. It turned out that Roy had gotten very interested in chicken mites because they were working on birds, and they also had found out about Smith, Blattner, and Heys's work and knew about our work, so he started working on mites and St. Louis and eastern encephalitis viruses. He couldn't duplicate any of their work. I had a colony of chicken mites by this time, and I tried to repeat their work but couldn't duplicate it. About this time there was another scientific meeting of the Society of Tropical Medicine and Hygiene, and I ran into Dr. Ed Sulkin from Southwestern Medical College in Dallas, Texas. He also had gotten all excited about mites, so he'd been trying to duplicate their work and also couldn't do it. He was working with St. Louis virus.

I then made a visit to Margaret Smith, and by that time Blattner and Heys had gone to some place in Texas; I never could run them down again. I said, "Margaret, we've got a problem. Three of us--Chamberlain, Sulkin, and I--have all been trying to duplicate your work, and we just can't do it. We've tried every way we can. It doesn't make any difference what we do, it doesn't work. Now, I've come to you. Can I get your chicken mites and your viruses to make a further attempt?" She said, "I don't have them anymore. Blattner and Heys took the virus with them when they went to Texas, and I think they took the mites too, but I don't have them anymore." I said, "Then what I want to do is to get the virus strain that you work with, because that might be different from ours, and I want to get mites from your source to work with. Can I go out to the chicken house where you started all this?" She said, "It burned down." Margaret's a nice person, who I knew was telling me the truth and she hadn't set a fire.

I said, "Margaret, we've got an impossible problem here, because we don't know what to do." I went over our data in detail with her, also what Chamberlain and Sulkin had done, and she said, "I don't understand it. I wasn't really that closely involved with all the day-to-day stuff in the lab, so I can't answer questions on this, but I just have the feeling that something's wrong with what we did, not with what you folks have done."

By this time all the textbooks were coming out, stating that the damn chicken mites were the basic reservoir and hosts of



St. Louis and eastern viruses and even implying they were for western equine because we had found it in field-collected mites. She said, "I'll publish a retraction." I said, "That won't serve any purpose." Finally Ed Sulkin, Roy Chamberlain, and I sat down together at a tropical medicine meeting and decided there was only one way we were going to kill the thing. We actually used that term, "kill the thing." We'd each spent five years trying to duplicate the mite experiments and couldn't do it. We were taking it very seriously. So we decided to publish three articles simultaneously in the American Journal of Tropical Medicine and Hygiene. Each of us would publish our negative findings, and we did.<sup>1</sup>

Hughes: That killed it?

Reeves: That killed it.

Hughes: Did anybody ever figure out what the Smith group had done wrong?

Reeves: No. We don't know what happened, and we couldn't duplicate what they had done. To be candid about it, at that point I came to the conclusion that I didn't care that much about the problem anymore. I'd spent all the time, five years, that I wanted to on it, and it hadn't solved our basic problem: what is the overwintering reservoir of these viruses? I think the experiments that Chamberlain, Sulkin, and Reeves and associates did in the laboratory were well designed. If there had really been something there, it should have come through. So we just had to make a decision, and we never, any of us, ever touched the problem again, and no one else has that I know of.

A number of things like that happened. Ed Sulkin came out with papers that said bats were the basic hosts of St. Louis encephalitis and that he could isolate St. Louis encephalitis virus from bats in Texas. We couldn't do it in California when we tried. Harald Johnson wanted to believe the Sulkin findings so bad that he could taste it, but he couldn't duplicate it.

Hughes: Oh, he loves bats.

Reeves: I know he does. He couldn't get St. Louis virus or western out of bats.

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<sup>1</sup>W. C. Reeves, W. McD. Hammon, W. H. Doetschman, H. E. McClure, and G. Sather, "Studies on Mites as Vectors of Western Equine and St. Louis Encephalitis Viruses in California," American Journal of Tropical Medicine and Hygiene 1955; 4: 90-105.



Hughes: He also wanted to get yellow fever virus and Bolivian hemorrhagic fever virus out of bats.

Reeves: He wants to get anything he can out of bats, and that's good. He finally discovered Rio Bravo and Kern Canyon viruses in bat salivary glands, which were new viruses. Rio Bravo is a very interesting virus. He found it at Rio Bravo School in Kern County. He was sure at first that virus was transmitted by mosquitoes, because there were so many mosquitoes right there along with the bats. Here are the mosquitoes sitting in this corner, here are the bats right alongside them. He said, "Boy, and all these mosquitoes are full of blood." I said, "Yes, but it isn't bat blood," and it wasn't. We tested them. If you opened up those bats, you might have found some mosquitoes in them. However, the mosquitoes are looking for blood a half an hour after sundown, and that's when the bats also are out feeding, looking for mosquitoes and other insects. The bats are feeding on the mosquitoes, but the mosquito hasn't been invented yet that can catch a bat. [laughter] Anyway, we had some very lively discussions about this.<sup>1</sup>

#### Mosquito Blood Meal Identification Techniques

Hughes: Is the use of blood meal tests for the identification of the host a technique that you developed?

Reeves: No. This was done earlier in the field of malaria epidemiology. A concern of workers was to determine how frequently *Anopheles* mosquitoes fed on people. I can't remember the names of the investigators, but Dr. L. W. Hackett discussed its use at some length in his book that was published in 1937.<sup>2</sup>

I discussed this briefly earlier regarding the recruitment of Dr. Tempelis to our team. If you are interested in whether mosquitoes have fed on human beings, you can take a blood sample from a person and put that into a rabbit, and the rabbit will develop antibodies to human blood. If you take serum from the rabbit and put that in a little capillary tube or even a large test tube, and you put human blood on top of it--layer the one on top of the other--at the interface there will be a precipitin

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<sup>1</sup>See Dr. Johnson's oral history for further discussion of these topics.

<sup>2</sup>L. W. Hackett, Malaria in Europe, Oxford University Press, 1937.

reaction, because the antibody is reacting with the antigen that's in that human blood. So it's a very specific reaction. You also can do this on a very micro scale in little capillary tubes.

They found out that they could take a blood-engorged mosquito and make a suspension of the mosquito in saline, put the capillary tube down into this blood meal that the mosquito has and suck it up, and then suck up a little of the antiserum against human blood so that the two fluids interfaced in the tube. If it was human blood, a white precipitin ring would form, so they called it a precipitin ring test. Now, this was very handy, because you can't go out and ask mosquitoes what they fed on, because they won't tell you. But you could go out and collect engorged mosquitoes, do this test, and determine what proportion had fed on people.

In the case of malaria, this was simple. It's a nice, straightforward answer. They didn't care too much what else the mosquitoes were feeding on. If they were feeding on some other form of blood, it was not a source of malaria. All they wanted to know was how frequently the mosquitoes were feeding on man, because horses, cows, birds, hogs, and dogs didn't carry malaria of humans.

Our first use of the precipitin ring test was when Dr. Fred Bang joined us in the Yakima Valley in 1941. He had come from the Reelfoot Lake project on malaria in the eastern United States. In some way or other he had learned about the precipitin test and had used it some to study *Anopheles* mosquitoes in the Reelfoot Lake project. So when he was in Yakima, he said, "Hey, let me take some of these engorged mosquitoes when I go back East again, and I'll see what they're feeding on. I'll use my precipitin test."

The malariologists had also developed some antisera that would react to dog, cow, other domestic animals, and birds. The problem was that when they made an antiserum against bird blood--let's say they used chicken blood to sensitize the rabbit--that antiserum would react to any species of bird. The reason for this is that bird blood is very complex, and they share many antigens which the rabbit doesn't have. If you take a simple example, humans and rabbits have many of the same antigens in their blood. So when you inoculate a rabbit with human blood, it doesn't react to any of the antigens that are already in its own blood. It only reacts to those that are specific for man or perhaps to some that man shares with a monkey, which he does. But they had a pretty specific antiserum. In contrast, if you inoculate a rabbit with bird blood, they share very few antigens. Every antigen that a bird has is unique to the rabbit, so the rabbit will react to all

of them. When you make an antiserum to chicken blood in a rabbit, it will react to any bird. That left us up a tree because--

Hughes: --you wanted to know the species.

Reeves: We wanted to know the species. So we developed a whole new system that would react to different species of birds. I brought Dr. Constantine H. Tempelis to our research staff--he's still here, as a professor of immunology--and he developed techniques by using the chicken as the antibody maker. The chicken could not react to the antigens it shared with a house sparrow or other bird species, and they shared a lot of them, so it would only react to the ones that the house sparrow had. Now, unfortunately, some of the house sparrow antigen cross-reacted, say, with the house finch. By a very sophisticated immunological method, he was finally able to develop very specific antisera and test techniques that would react only to house sparrow, house finch, blackbird, and so on.

So we had to modify the whole system. We had started to get the information during the bird malaria study, when we showed that many species of birds had malaria and the parasites were transmitted by *Culex tarsalis*. We could have done the same thing just by taking *Culex tarsalis* blood meals and making a smear of them. If we had found nuclei in their red blood cells, we'd have known it was a bird, because mammals don't have nuclei in the red blood cells. Camels do, but there are no camels in Kern County. [laughter]

Now, if you were doing humans, you couldn't just look at red blood cells, because any mammalian cell is going to look like a human red blood cell because they don't have nuclei. We didn't even bother with this, because in many ways it was easier to test the sample with a precipitin test than it was to make a blood smear out of it.

So we refined and extended the technique for studying mosquito blood meals, which has become adapted now and used all over the world. There are new techniques that improve further on the test.



## Associations with the Centers for Disease Control

### Encephalitis Programs

Hughes: Dr. Hardy said that your encephalitis program was the model for the CDC and throughout the world.<sup>1</sup> Could you amplify?

Reeves: Well, you like to think that's true. [laughter] I guess we have to go back to things I was discussing earlier today. In 1946, '47, when we developed the collaborative program with the Centers for Disease Control, they had no program on encephalitis. They sent people here who worked with us and within our system, and then they developed separate programs.

This wasn't done in a very simple or straightforward fashion, because they had several different groups in CDC that started developing programs. They had a program in Montgomery, Alabama, and Dr. Morris Schaefer was the director of that program. Roy Chamberlain and Danny Sudia were there. Because they were the laboratory for the CDC, they had an obligation to do diagnostic work. With the two entomologists who were there, they soon became involved in field and experimental work very similar to what we had been doing.

Hughes: When was this?

Reeves: They were certainly up and running very well in the 1950s. At the same time, they had an epidemiology branch in the CDC. Anytime there was an encephalitis epidemic, they would get involved in the diagnostic aspects in the sense of getting blood samples and studying the disease epidemiologically. So CDC had the laboratory branch in Montgomery, and the epidemiology branch had a separate field station that was first in Kansas City and then in Greeley, Colorado. The technology branch of CDC had a field station in Logan, Utah. Anyway, it was very complicated. There was also a group from the National Institutes of Health at the Rocky Mountain Lab--Dr. Carl Eklund and his group. So there were four programs in the Public Health Service, and our program. Here we were, we had all these different stations developing an interest in encephalitis, and we were communicating with each other pretty well; at least we were communicating with them.

About this time there was a real problem between these four different programs of the CDC and NIH. They were competing with

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<sup>1</sup>Interview with James L. Hardy, Ph.D., November 16, 1990.

each other. They were vying for funding, they were competing for territory and who should and could investigate which epidemic. They really got in each other's way, and they were fighting about, "This is my geographical territory." Carl Ecklund was saying, "All this territory, the Rocky Mountain area and so on, is mine," but now there's a group in Greeley, Colorado, and they say, "Well, we're in the Rocky Mountains. How can you say that?" And then there's a group from epidemiology. If there's an epidemic, they move in, and pretty soon there would be a need to send specimens to the Montgomery lab. It was just a mess.

Your original question was whether our project was used as a model. I think so, as far as methodology is concerned, type of staff that was needed, and the objectives. Our project certainly didn't define what everybody had to do, but it certainly was a model of collaboration, it had a wide base, and it defined what was needed in the way of professional concern. I think the most successful programs were based on those premises. A lot of programs, of course, were completely separate. I mean, the Rockefeller Foundation worldwide program wasn't directly connected. Maybe some of the Foundation's field programs were influenced by our program, but I wouldn't even pretend to take credit for that.

The Foundation's basic program was to identify the arboviruses of the world, to determine their relationships to disease, and to collect all the viruses. Largely due to that program, we now know there are five hundred and some arboviruses in the world. The establishment of the Foundation's big laboratories and headhunting for viruses all over the world was responsible for that advance. We were doing the diametric opposite from headhunting for viruses. We decided early on that our field was western and St. Louis encephalitis. Any other virus, until it was proven to be an important human disease, was really of no particular concern or interest to us. So we used laboratory techniques that would specifically isolate those viruses and would miss other viruses. We knew they would miss other viruses. We didn't care. If we hadn't done that, we would have been submerged by a plethora of viruses.

Now, the original question was, how is the CDC modeled after us? They were modeled after us in the sense that what was really causing much of the turmoil was the fact that they were all working on the natural history of these viruses and trying to further elaborate what was known about these diseases. So we were also in a competitive situation. The CDC didn't want to assign anybody anymore to work in our lab. They didn't need us anymore; they had their own program.

### The CDC-NIH Meeting in Hamilton, Montana, 1952

Reeves: The powers that be in the Public Health Service, CDC, and NIH, decided that some way or other this had to be resolved. So they called an emergency meeting in Hamilton, Montana, in January 1952. They brought in three consultants from outside of the Public Health Service. NIH brought Dr. Meyer and Albert Sabin in as consultants. CDC brought me in as their consultant. Bill Hammon didn't go, as he had moved to Pittsburgh.

Hughes: Why Albert Sabin?

Reeves: Because he was a big shot and had worked on arboviruses.

Hughes: But wasn't he working on polio?

Reeves: Not to the extent he did later. At this time they were having big arguments about a technique Albert had developed, the HI [hemagglutination-inhibition] test which had become very popular for studies on these viruses. He was doing this work at Cincinnati. At this stage he was still doing a lot of work on dengue and HI tests for arbovirus work generally. As a matter of fact, I'd say that at this stage, for practical purposes, Albert was not really working on polio. He didn't start that until the sixties. I didn't say he wasn't interested; I said he wasn't really working intensively on polio until later.

Dr. Meyer and I traveled to Montana together, and we decided en route what the strategy should be to get these people to work together and so on. We decided that he'd been paid to go up there by NIH and I'd been paid by CDC, but when we got to the meeting we'd switch roles; I'd become the defender of NIH and he'd become the defender of CDC. [laughter] That would confuse everybody, which it did. The NIH people couldn't understand, "Why is this guy Reeves defending us, and why is CDC getting all this help from K. F. Meyer?" But it all worked out very well. They decided the only answer was for them to stop getting in each other's hair and have annual meetings of all the workers.

What it really meant was that yes, they all knew what we were doing. I won't say who did what first or anything else at this stage, because there were some very competent people working in all these areas. But I still think our basic program was the model.



### The 1952 Report to the CDC<sup>1</sup>

[Interview 5: February 7, 1991]##

Hughes: Please tell me how you went about collecting data for the CDC report that you wrote in 1952, and what kind of data you were trying to collect.

Reeves: In April 1952, after the meeting in Hamilton, Montana, I got a telephone call from Justin Andrews, director of the CDC, who said, "I've had a meeting of the various staffs in epidemiology and virology and vector studies here"--the three different parts of CDC involved--"and they agreed that they want a review done. I've talked to some of the people outside, and we want you to do the review. We want you to travel around and visit all of the laboratories in the United States that are doing any work on the encephalitis viruses. I want you to write an administrative review for me about what's going on, what the findings are, what the main questions seem to be. I want it to be clear that it's an administrative review so that no one will be holding out information in the sense that it's going to be misused and published for them or something of this kind. You can go out and do that; you may be the only person they'll listen to and will allow to do it."

Well, this sounded like a very challenging thing to do, but I said, "I have a big field project set up for this summer." Dr. Andrews said, "You've got our people there. You've got Dr. Bellamy in Bakersfield, and he can do that job. We've sent you a vertebrate zoologist, and we'll give you some more help if you need it. I want you to sign a contract to do this."

I talked with Dr. Meyer about it, even though at that time I was for practical purposes spending all my time in Berkeley. As a matter of fact, we'd moved to Walnut Creek, and I just had a small laboratory at the Hooper Foundation and had moved most of the laboratory to Berkeley. Dr. Meyer said, "You ought to do it," so I signed the contract. That was in April of 1952.

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<sup>1</sup>W. C. Reeves, "Report on the Current Status of Research and Knowledge on the Arthropod-borne Viral Encephalitides in the United States," an administrative report not published, duplicated by CDC, February, 1953. A copy of the report is on deposit at The Bancroft Library.

I learned later from Dr. Justin Andrews that he had decided, "Look, if I'm now going to really have a program in encephalitis in the Centers for Disease Control, I cannot make an administrative decision about what we should do without having a real review of what's known about encephalitis, what research is now being done in encephalitis in the United States, and where can we be most important, where can we play the largest role."

So I started making visits to Montgomery, Alabama, and all over the country. A lot of names that are well known now were working on encephalitis at that time. Dr. Albert Sabin was in Cincinnati. I'd have to go back to recall all the forty places I visited. At the Rockefeller Foundation were people like Max Theiler and Jordi Casals. I visited them; I reviewed their work. I covered the country. I went to Alabama, Kansas City, Salt Lake City--CDC had a program there--Hamilton, Montana, the army at Washington, D.C., and Camp Detrick, Maryland. I went to the New York State Health Department, Minnesota, Illinois, Texas, New Haven; I covered everything. I visited Bill Hammon and Jonas Salk in Pennsylvania. So I covered the waterfront. The report has it all.

Hughes: What kind of details were you incorporating?

Reeves: What they were doing.

Hughes: In how many specifics?

Reeves: In detail. I actually got original data about serological results that had never been published, that showed the viruses were in places that we did not know they were in at that time. I got the latest information on the development of new tests for diagnosis, on new methods of isolating viruses. We got filled in on what was going on currently in yellow fever work at the Rockefeller Foundation. They were just getting into the separation of all the new arboviruses that were coming in from their field programs in various parts of the world.<sup>1</sup> So it was a very detailed report of what a large group of workers thought were the questions to be answered and the types of data needed to answer those questions.

Hughes: Were people forthcoming?

Reeves: With a few exceptions, they were completely forthcoming. I had a little difficulty with some people. I won't name names, because some of those people are still around. I had some directors of

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<sup>1</sup>See Dr. Johnson's oral history for a discussion of the Rockefeller Foundation's arbovirus program.

programs who didn't want me to talk to any people on their staff unless the director was present. Sometimes a worker would start talking about something, and you could see a signal: "Don't talk about that anymore." When that happened, I felt very duty-bound. After all, I was working for their boss, and I felt duty-bound to find out what they weren't talking about, so I would find one way or another to talk to the staff when the boss wouldn't be in town. I'd go back and visit again, and the minute I'd show up, the workers would be willing to tell me anything I wanted to know.

Hughes: Were you reassuring them that your report was not going to be published?

Reeves: I was assuring them in every way I could that I had orders, and it was going to be an administrative report. The report says it very clearly on the cover: "Materials in this report are for administrative use only and not for publication." That was not violated. We are violating it currently, but I received permission from CDC to do so a few years ago. I came across a copy of this report in my files when I was working on the monograph, and I reread it. I thought it was rather interesting historically, as it covered the state of the field in those days. There were forty or fifty laboratories in the United States working on encephalitis, and many were in universities. That is a fantastic contrast with today. In today's world I wouldn't have ten places to visit in the United States that are working in detail on these viruses. But it was a new field then, and it was exciting to a lot of people.

The California Mosquito and Vector Control Association considered the publication of that report but found it would be too expensive.

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Hughes: No data from this report was to be published?

Reeves: No.

Hughes: Because that's the only way you would have gotten anywhere?

Reeves: Right; people weren't going to let me publish their data, and I wouldn't have taken on the job if that had been the case.

I was on this project for three months, as it took a long time to visit all the laboratories and to make repeat visits to pick up additional details. Several times all my luggage got lost. I remember one time I was gone for ten days and wore one wool suit, one shirt, and one pair of shorts, as they never found



my suitcase. When I got home it was sitting at home; the airline had sent it back here. That trip was to the southeastern United States, and July is a nice time of year not to be there in warm clothes.

But the most interesting experience I had during that assignment was in late May, early June. I was sitting in Dr. Andrews's office, and his secretary came in and handed him a telegram. He looked at it, and he looked at me and said, "What the hell are you doing here? Why aren't you in California?" I didn't know what he was talking about. I said, "Dr. Andrews, you told me this was my job. I'd rather be in California." He said, "Why would you rather be in California?" I said, "It looks like we're having an epidemic of encephalitis, and I'd like to be there." He said, "That's what this telegram is about. It's from Dr. Wilton Halverson, the state health officer of California. He has requested that I send him three physicians, two engineers, and five entomologists to assist in the investigation of the epidemic, and it seems to me you ought to be there instead of my sending these people." I said, "I could never do what all those people would do. Send them. But I'd be glad to go back if you want me to. You can pay me to go back there and do it." "No," he said, "I want you to finish this job." He said, "I'll send all the help they need," and he did. I don't know how many people they finally sent. It's all written up in publications by the state health department. That was the largest epidemic of encephalitis in people that we've ever had in California.

Hughes: Did you miss all of it?

Reeves: Well, for practical purposes. I was there July 6 or some date like that when the Bakersfield earthquake hit. Any time I was back in California I would tear down to Bakersfield as fast as I could get there to catch up on what was going on. I would pack all my notes from my last trip with me so that I could work at night on editing them. When I'd go to Bakersfield, I would go to the laboratory and into the field. I'd go to the state health department to keep up on what was going on as much as I could, because that also was a part of my report at that time. But it was a busy time.

Hughes: How, specifically, was Andrews or CDC going to use this information?

Reeves: They were developing a program in three branches of CDC, and they hoped to coordinate the three programs, but they didn't want to duplicate services or activities that other people were already doing. They wanted to build an organizational program that would meet a need in state services, which was a primary objective of

the CDC programs. They were delegated by the Congress to be the state service organization of the United States Public Health Service. NIH was still to be the main research base for the Public Health Service. CDC was trying to figure a way to have a unit that brought together the various interests in CDC so they didn't have three separate programs.

At that time they had three autonomous activities. They had a program in their laboratory section which was completely autonomous from that in the epidemiology section. The epidemiology section said they also had to have a laboratory, that they couldn't do work on the epidemiology of the disease without doing diagnosis, and they didn't want to send their material to the other people; they said they wouldn't work with them. That's one reason that led to the laboratory being moved from Montgomery to Atlanta so that it was under the same roof with epidemiology. Their entomology section had a program in Salt Lake City which was moved to Logan, Utah, and then went to Greeley, Colorado. They also had some way or other to have a laboratory with that outfit. There was a real need to integrate these programs by direction from above.

Hughes: Which they did?

Reeves: Eventually they were all integrated into one program in Colorado, which was in Greeley originally and is now in Fort Collins. So epidemiology, virology, and entomology are all under one roof.

#### Viruses Isolated in Kern County##

Hughes: A little earlier you were talking about twenty arboviruses in Kern County.

Reeves: Yes, we know of twenty-some viruses in Kern County or other areas of California, because when techniques were changed later we and others isolated, many new viruses. But originally, we just stuck with adult mice for virus isolation. We didn't mess around with baby mice and tissue culture and other fancy techniques as they evolved. If we did, we were going to diverge off from what we were really after.

Hughes: Because of course there would be other ones, too.

Reeves: I have mentioned some viruses that are not transmitted by arthropods but are antigenically related to them and are in the same environment. There are only a couple of them. As an

example, Rio Bravo bat salivary gland virus, isolated by Harald Johnson, is included because it's right there in the same environment as the arboviruses, and it's very closely related to St. Louis encephalitis. When we're doing antibody studies on animals and get reactions to St. Louis virus, we have to be sure that it's not Rio Bravo virus that's affecting animals other than bats.

Then we have Modoc virus, which is another virus that Harald Johnson isolated from the mammary tissues of a *Peromyscus* mouse. This also is a virus related to St. Louis virus and to Rio Bravo virus. It also is in the flavivirus group. Modoc virus occurs in Kern County, but it's not carried by arthropods at all. It's transmitted directly from mouse to mouse. So we have included viruses like those within the arbovirus group just because they're so close virus-wise to St. Louis, which is mosquito-borne. They infect many of the same mammals to some degree.

There also is a group of viruses we've isolated from *Culicoides* gnats. These insects are somewhat related to mosquitoes. They're very small midges that are bloodsuckers just a fraction of the size of a mosquito. We went after them because we were interested in whether they might also be vectors for western and St. Louis viruses. They were very common in Kern County, but it turned out that while there were black flies and sandflies, *Culicoides* species predominated.

*Culicoides variipennis* is the principal vector of bluetongue virus in sheep, which is a very common infection in California and a real veterinary problem. It also is the vector of hemorrhagic fever of deer in California, which is a problem in wildlife. We didn't include the methods that would isolate bluetongue or hemorrhagic fever viruses in our studies. We would have had to put a whole new virus isolation system into the laboratory to do so. So we used the systems we had used to isolate western, St. Louis, California, and other mosquito-borne viruses when we tested *Culicoides*. The first thing we knew, we had isolated three new viruses out of these gnats. We didn't get any western, St. Louis, or California virus out of them. We did get three other viruses--Buttonwillow, Main Drain, and Lokern. These are still largely viruses looking for a disease. Main Drain virus has been found in the brain of an encephalitic horse. That sort of thing happens occasionally. You must admit these viruses were given nice names--Buttonwillow virus; you don't even know what a buttonwillow is, do you?

Hughes: No.



Reeves: The buttonwillow is a tree that grows in Kern County, and the original settlers found that they could take the little hard seeds that come from this tree and make buttons out of them. They're just the size of a button. The town of Buttonwillow is named for the tree, and that is where we first isolated the virus.

Hughes: Main Drain?

Reeves: Main Drain is an area that forms the natural drainage that goes from the Kern River to the Tulare Lake basin. Lokern virus comes from another region in that area. We put these place names on the viruses because it's become sort of the thing to do. Namely, any new arbovirus that's isolated anywhere in the world gets the name of the place where it was found.

Hughes: Where it's first isolated?

Reeves: Where it's first isolated, yes. It's as good a way to name them as any, instead of naming them after people. So I've always wanted to find a new virus in Pumpkin Center or Weedpatch in Kern County or in Mosquito Lake in the Sierra Nevadas, but I never have.

The Mosquito Abatement Districts and the Vector Control Section,  
California Department of Public Health, 1948

Hughes: I saw a reference to a blowup between the mosquito abatement districts and the Bureau of Vector Control of the California State Department of Health.

Reeves: As I recall, this happened back in 1948. As I said earlier, the state legislature in 1945-46 had agreed that there were two disease problems they were interested in: one was malaria and the other was encephalitis. They had allocated money to the State Health Department, not only to initiate research on these problems but also to have a subvention program that would add money to the local budgets for mosquito control.

A number of control districts had been formed at that stage; I suppose there might have been close to twenty of them in California in the postwar period. The state legislature included, in the money that they'd allocated, monies to the State Health Department to provide subventions to increase the money for local

This gave the State Health Department a pretty good wedge to start telling local mosquito abatement districts what they should do and how they should do it, which was not improper if they wanted the money. At the same time, the local mosquito abatement districts considered themselves to be completely autonomous from state government. "Yes, we'll take the money. Yes, we'll develop this. You can tell us standards, but you can't tell us everything we're going to do and how we're going to do it."

By 1948, this program had gotten to the point where a single engineer in the state health department had responsibility for it, a Mr. Arvie Dahl. He was brought in as an engineer and had had considerable experience in mosquito control elsewhere; I don't remember where he came from. He was a very aggressive person, and he worked very hard at setting standards and raising standards for light trap collection, records of where they sprayed, training programs for employees, standards for a manager, and so on. By this time, Dahl had also hired an entomologist, Mr. Richard Peters, who took over from Dahl later.

Anyway, Dahl was throwing his weight around at a pretty good rate, and the mosquito abatement districts decided they didn't like this. So they were going into revolt. They held a meeting and invited me to come. They made it very clear that they were after Dahl's scalp and that they were going to get rid of him because he was being too bossy, and they thought he was setting himself up as a czar for all the mosquito control in California. These people were pretty proud of what they had, and there were some very good managers in the group. I was working very closely with a number of them; I knew them all very well. I'd met with most of the boards of trustees that governed the policies in their districts, and I wasn't in disagreement with the position they were taking.

I attended the meeting and listened to them, and then I came home. Then I got a phone call that they wanted me to go to the state health officer and present their position and their problem. I said-I didn't think that was my job, and they said, "We want you to do it." It was obvious that if I didn't do this there were going to be even more problems, and I'd be losing my contact with them.

Hughes: Why did they pick you?

Reeves: I don't know. They said I could do it. I don't know any more than that. When they said I could do it, I guess I had to.

So I went to Dr. Wilton Halverson and Dr. Malcolm Merrill, who were the health officer and assistant health officer in the state, and met with them, Arvie Dahl, and Frank Stead, who was the head of the whole Engineering and Vector Control Section and also a good friend of mine. We all met in a room, and I just laid the problems on the table. Halverson said, "Mr. Dahl, we're going to have to change our ways, because the State Health Department shouldn't be having these sorts of relationships." So they sat on him pretty hard, and he was only here about six months or so after that. He left, and then Mr. Peters was put in charge.

There still are some problems in this regard, but they are minor, and there always are problems when a state organization sets standards for a local organization. Local organizations are responsible to their boards of supervisors, boards of trustees, and local citizens, not to the state.

But actually it worked out very well, and the Vector Control Section under Mr. Peters became a very strong force in extending the number of mosquito control districts in the state, participating with them very closely, developing better standards for training, and this type of thing. The Vector Control Section is asked to send representatives to the trustee meetings when they need advice, and the Vector Control Section keeps people from mosquito abatement districts on their advisory groups. It has worked out in time, and most of the time it's a pretty peaceful situation. It was very difficult at that earlier time.

Hughes: Would you care to make a comparison between the California mosquito abatement program and those of other states?

Reeves: I really am not that well informed on all the details of the other state programs. I think I can say with some assurance that there are three or four programs that stand out.

The Florida mosquito control program is a statewide program. The state has a lot of responsibility for the local programs as far as money is concerned and so on, but they also have local autonomy. It's a good program and includes a large research program which is carried out by state people at Vero Beach.

The New Jersey program is what you might call the original one. I think they had a control program a few years before any was started in California. It was started primarily as a state program for salt marsh mosquito control, not for disease control. The New Jersey Mosquito Extermination Commission is still a very strong organization; they have their annual meetings and so on.



There are mosquito control programs in all of the western states. Utah is the only one other than California, New Jersey, and Florida that has a well-organized state mosquito control association. Others have regional organizations for meetings. Utah has a strong program in the heavily settled areas--Salt Lake City and places like that. There also are programs in Louisiana and other areas that I'm really not that familiar with because I haven't worked there. They all come together in the American Mosquito Control Association, which was formed, I guess it was, back in the fifties. This is an organization for people from any place in the United States or elsewhere to get together for an annual meeting to talk about mosquito control as an overall national interest and concern.

#### Expanding Interests of Mosquito Abatement Districts

Hughes: Is disease control a primary interest?

Reeves: In today's world, disease control is a principal interest. There are two main aspects of mosquito control programs--pest control and disease control. Pests aggravating people also can cause real physical health problems. Until very recently, these organizations were only concerned with mosquitoes as pests and vectors of diseases. Currently many of these districts are expanding their area of responsibility and interest. They're expanding into other insects that are pests, like fly or wasp control. Some are even extending their interest into rat control, because rats are becoming a major health problem. Health departments aren't necessarily the best organizations to be concerned with this, because they may have more of a limit on their tax base than do mosquito control districts.

Hughes: What about the fruit fly?

Reeves: No, no, they're sticking to insects that have some relationship to human health. Some of the districts are now getting very much involved with Lyme disease, because Lyme disease is another health problem that's carried by a vector, in this case ticks, and it's a bacterium that causes it. So some of the districts are now including Lyme disease and its control as a part of their activities. Some are expressing interest in control of the African killer bees when they invade California.

None of them have gotten into control of the veterinary diseases that are transmitted by vectors, with the exception of encephalitis. But diseases like bluetongue in sheep, tularemia,

and plague they haven't gotten into yet. I anticipate that many mosquito control districts will become vector control districts and will even include animals in their realm of activities. The California Mosquito Control Association changed its name in 1977 to the California Mosquito and Vector Control Association.

Hughes: Does that mean probably more intervention from the state?

Reeves: I doubt it. The handwriting on the wall says that the state is not going to be able to provide all the support that local districts really would like to have to maintain surveillance programs and other activities. As a matter of fact, the mosquito control districts now are giving some money to the state health departments to increase the surveillance of encephalitis activity. The virus lab at the State Health Department did not have enough laboratory positions to really do the job the control districts wanted. Some mosquito control districts and local health departments are even setting up their own virus laboratories. I question the wisdom of some of this decentralization because of the type of security facilities, the class of person you have to have, and quality control to get good data.

Hughes: You mean too much money is required to get what is needed?

Reeves: It's not cheap to develop a Biosafety Level 3 laboratory that will protect the technicians from dangerous pathogenic organisms they are to be handling. It's not cheap to get people who are not only able to run some sort of a routine test, but if it's not going right to recognize the fact that they're making mistakes. This is where California is unusually lucky to have a laboratory like the Virus and Rickettsial Disease Laboratory that Dr. Lennette organized and that Dr. Richard Emmons is continuing, and the state bacteriological laboratory for Lyme disease and plague to carry out this sort of service.

I just learned a couple of weeks ago that the five new positions that were established in the State Health Department two years ago for surveillance and diagnosis of Lyme disease have been cut out as of next year's budget. The control districts are taking a rather dim view of this. Lyme disease is now the most common arthropod-borne disease in California. There are more recognized cases of Lyme disease than we have of encephalitis, Colorado tick fever, relapsing fever, or plague. So the districts that are going into that problem need that support. But one way or the other, the state's going to have to resolve these issues. Everybody in this state is going to have to agree on what they need and what they want.

As we go into the next century, I visualize that we're going to have some level of vector control in every area of California because of the population movements that are now taking place. There are going to be a lot more people in California in the next century. The population in the Central Valley and southern California will double between now and the year 2020. Most of those doublings of population are going into what are now relatively unpopulated areas where the vector-borne infectious organisms are common. We've already had plague cases in suburban Los Angeles areas where people have moved out into the foothills.

Hughes: Is the state anticipating this problem?

Reeves: Yes. I gave two papers<sup>1</sup> this year at the California Mosquito and Vector Control Association meeting just on this problem, because they wanted me to talk about it. Yes, I think everybody agrees that it's going to be a major problem. Right now the movement of people into the Sacramento and San Joaquin valleys is becoming overtaking. It's taking farmland out of production. It's putting people right into the environment where the diseases are present in their natural cycles. There are thousands of people moving to the foothills of the Sierras, and they're dangerous areas, because generally there's no vector control. In Riverside and San Bernardino counties, an extensive desert area is being developed and urbanized. It's going to be a problem.

So I anticipate that state and local organizations are going to get closer and closer together in their relationship. There will always be problems, but as long as they meet and talk to each other, it will work out.

To get back to your original question, I don't want to be asked again to be the middleman. I don't want to have to go to the state health officer, as I find it more difficult to do it now; I don't have the relationships with state health officers that I did before.

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<sup>1</sup>W. C. Reeves, "The Changing Vector-borne Disease Picture in California and the Western Hemisphere in General," Annual Meeting, California Mosquito and Vector Control Association, January 29, 1991, Sacramento, California.

W. C. Reeves, "Population Growths in California and Their Impacts on Disease Transmission," Annual Meeting, California Mosquito and Vector Control Association, January 28, 1991, Sacramento, California.



Changes in the California Department of Public Health

Hughes: But you were the middleman for much of your career.

Reeves: What you have to realize is that the state health officer back in the forties and fifties was responsible for public health in the classical sense. He wasn't responsible for medical care, he wasn't responsible for all the other aspects of health that are now in what is called the California State Department of Health Services. So the State Health Department, as it was, was abolished. Governors [Jerry] Brown and [Ronald] Reagan abolished the State Health Department, and they abolished the state board of health, which was the advisory group that set a lot of the policy and the new laws for public health. They don't have a state board of health anymore.

Hughes: What was the rationale?

Reeves: Reagan and Brown discovered that they didn't have control over who the state health officer was; he wasn't solely a political appointee. As a matter of fact, Governor Warren, in his wisdom, had made it a state law that the state health officer could not be replaced by a governor for at least one year after a new governor took office, because Warren felt when you had a competent person as state health officer, that that person should be continued.

Hughes: Who did appoint the state health officer?

Reeves: It was pretty much handled by the state health board advising the governor. They did a search, found a good candidate, and they had a series of excellent health officers.

Hughes: I know from talking with Dr. Lennette that the California State Department of Public Health was virtually second to none in the early days, but I gather that it's very much changed, thanks to Reagan and his successors.<sup>1</sup>

Reeves: Well, it certainly has changed. I don't know where I would rate California now as compared with other state health departments, because I no longer have that much contact with the other state health departments. It will still certainly be very high in its competence. It's much broader than it used to be. It used to be that many of the environmental problems that are now inherited by the State Department of Health Services on radiation levels,

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<sup>1</sup>See Dr. Lennette's oral history.

childcare, medical care, hospital standards--an endless array of activities, some of which have nothing to do with prevention of disease--are very different from what they had before. Earlier they had a cozy sort of situation. They were dealing with disease problems and their prevention in populations. They weren't having to put their resources into activities such as the health program for medical care for all the people in California, on the one hand, and still have enough money left to do their other programs. As a matter of fact, there have been periodic efforts to divide the department and put the medical care aspects as a completely separate activity, just as education in schools is separated from other activities.

Hughes: You mean the activity of a different organization?

Reeves: That's right. Have new organizations in the state that would be dealing with these things and have the health department concerned only with disease problems and their prevention, as they were historically. I doubt that will happen, because the same thing's happening at all the county levels, in other states and nationally. We no longer have county health departments as we had them classically. You have county health services programs that are concerned with medical care and all the other activities. The difficulty is that you are responsible for preventing diseases at the same time you're having the responsibility of taking care of all the diseases that are already there and that haven't been prevented. Disease represents a failure of the system. The two activities are really incompatible in some ways.

Hughes: It takes a different sort of mental outlook, I would think.

Reeves: Oh, yes. The fact is that you may get somebody running programs who is concerned primarily with medical care but responsible for health services of all types. Their focus of knowledge and concern may settle on the administrative aspect and not on the more scientific or preventive aspects of the program.

So we've had a real change of posture in this whole thing, and I'm glad it's not my responsibility. The same thing's happening within the schools of public health; they're getting into many new areas now. Almost the majority of the students are in health care and related activities and not in the classical preventive and science areas. That's not a criticism; we need such people.

Hughes: That seems to overlap with the medical schools.

Reeves: The medical schools don't give a darn about training people to run health care programs. Most medical schools are interested in

training doctors to be doctors. They think they have a major loss when one of those people goes into an administrative position for health care. They're not set up to train such people. As a matter of fact, the program we have now between the School of Public Health and the School of Business Administration represents this new direction and is indeed training people to be administrators of health care programs.

### Visitors

Hughes: I don't know how we got to where we are, but let's switch again to visitors and trainees.

Reeves: You asked me in one of the early sessions when we knew we'd arrived, in the sense of people knowing that we were here and what we were doing. I think one way to judge that is when people started arriving on the doorstep and saying, "We want to know what you're doing. We want to see your program. We want to learn about this technique or that problem. We want to send somebody for training so they can come back and do the sorts of things you're doing." This first became very apparent in the 1940s, when we started getting visitors from various parts of the world who were coming here just for that purpose.

Reeves: In the post World War II period, one of our first visitors was Dr. Manaba Sasa, who came from the National Institute of Health of Japan. He was trained in medicine, but his primary interest in life was bugs, insects. He had this interest, which he still maintains to this day. As a matter of fact, when he retired he went back to studying a group of insects that had no public health significance whatsoever, just because he was interested in them. He was the first visitor from Japan after the war and had been in charge of their malaria control program in the Pacific. He spent a number of weeks in Bakersfield learning the techniques we were using. He took them back to Japan and put them into operation in their programs.

We had people from some thirteen different countries who came to us in the period between 1949 and 1960. We were flooded with them, and they would come not just for a week or two to see what we were doing, not as casual visitors, but some would come to spend a year or more in order to go back to their country and set up the same sorts of programs.

Hughes: What did you do with them?



Reeves: Put them to work. The best way to learn these things is to do them. We would put them through both field and laboratory aspects--some people only the laboratory, some people almost all of their time in the field, depending on what they were going to do when they went back. But everyone got rotated through the whole system. In almost every instance, these people would have some small project that we had been able to set up for them, and in the end they would have a publication with us on that study.

Hughes: Were these senior people?

Reeves: No, most of them were junior people, but most of them rose to be senior, responsible people. For instance, Dr. P. K. Rajagopalan came from Poona, India. He was sent by the Rockefeller Foundation Laboratory back in the fifties. When he finished up here, he went back to the Poona laboratory<sup>1</sup> for a while but then was in charge of the Vector Control Research Center of the Indian Council of Medical Research in Pondicherry, India, which is the leading medical entomology research laboratory in that region. We still get their annual reports and hear from him periodically. He recently retired.

Dr. J. David Gillett came from the Virus Research Institute in Entebbe, Uganda. He's from England, and he came in the mid-fifties and spent a year with us. I believe the Rockefeller Foundation supported his visit. He was well known as an entomologist, but he came and spent a very happy year working with us on mosquitoes in Kern County and other activities. Later, in one of his books on mosquito biology<sup>2</sup> he included a lot of the things that he'd done with us.

Dr. Ian Marshall came from the National University in Canberra, Australia, and spent two years with us in the early 1960s working on mosquitoes and encephalitis. He also worked with Dr. D. C. Regnery of Stanford and demonstrated that the California brush rabbit was a reservoir of myxomatosis, an important disease of domestic rabbits. On his return to Australia he developed the WHO Regional Laboratory For Arboviruses and did important work on mosquito-borne diseases in Australia.

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<sup>1</sup>For the early history of the Rockefeller Foundation's laboratory in Poona, see Dr. Harald Johnson's oral history.

<sup>2</sup>J. D. Gillett, Mosquitoes (London: Weidenfeld and Nicholson, 1971).

Reeves: A wide variety of people came from Australia, New Zealand, India, England, Israel, Trinidad, Switzerland, and many other countries. They came in and went through the mill of working with us. People who knew no entomology learned some entomology with us, and people who didn't know anything about virology learned virology. In addition to that sort of training, every summer for a period of years the State Health Department had a program where they took on twenty or thirty medical students for the summer to give them an experience working in some aspect of public health. So as many as four or five of those students were sent to Bakersfield for the whole summer, and we used them on flight-range studies on mosquitoes and follow-up of encephalitis cases in the hospitals. Whatever we wanted them to do, they were there for experience, and we gave it to them.

Sometimes you almost had too many people. We had as many as eighteen people at the encephalitis laboratory in Bakersfield at one time, and it became crowded.

The reason that you knew that you were making some sort of a splash was the sheer number of short-time visitors that you had. Even starting back in the forties, it would be an unusual year when we didn't have anywhere from five to ten visitors. These were not just visitors from overseas who were coming but also the directors of the programs for Centers for Disease Control. The CDC staff visited us because they had a program here, and they wanted to be able to get up in front of congressional committees and tell them what they were doing. So long before they had their own program at CDC, Justin Andrews, Dr. George Bradley, who was the head of the entomology program, Roy Fritz, who later was with WHO, and people like that were here. Over a period of years we had four directors of CDC come through Bakersfield.

Some of them were rather shocked to find out what we didn't have at Bakersfield, because we had two little old adobe buildings that were built during the WPA days that didn't have any steel reinforcement, as we found out when we had earthquakes. It was very primitive. They envisioned we were going to have a fancy setup. On one of these visits, Dr. S. Simmons was so impressed that he arranged for CDC to ship us a new Butler building that served as our insectary for many years.

Hughes: Were they really interested in the nitty-gritty of research?

Reeves: They wanted to know exactly what we were doing. These were good scientists in their own right, and they wanted to make sure that they were getting the most for their money from the people they assigned here, and they wanted to know the details. We had to write quarterly reports about what we were doing. I still have



all those quarterly reports, but I don't guess you want to read them.

In addition, all sorts of people from the Hamilton, Montana, laboratory would show up who were working on encephalitis. In 1952, during the middle of the epidemic, I suddenly got word--I was in California at that particular time--asking if it would be possible for a group from CDC to come and visit us. It was Roy Chamberlain of the Atlanta laboratory, Dr. Clarence Sooter from Greeley, Colorado, and Dr. Adrian Cockburn, who was an Englishman working for CDC, along with Dr. John Rowe from Kansas City.

So we had this group of people who came to see what we were doing during the encephalitis epidemic. Dr. Cockburn, who was the physician, was over at the hospital. He liked to take movies of cases. When they came to Bakersfield, we'd already had the earthquake at Tehachapi, which had made the newspapers. So when the visitors came, they said, "We came to see your project, but none of us have ever felt an earthquake, so you have to give us an earthquake." I said, "Fine. No problem." Big joke.

The last afternoon they were there, they were sitting around this conference table, and the Bakersfield earthquake hit. That wasn't any joke. Three-fourths of Kern General Hospital was destroyed. Dr. Cockburn was up there taking a picture of an encephalitis case. There were a hundred proven cases of western encephalitis in that hospital that summer. We were up to our ears in encephalitis cases. All of a sudden things started shaking in the hospital and the oxygen tank went over, and Cockburn said, "What did I do? What did I do?" The guys around the table at the laboratory started trying to get out of the room, and I said, "Sit down." And they did, until it was over. Then the ambulances started to come into the hospital and so on, and the visitors we had couldn't get out of town fast enough. They'd seen an earthquake now, and they didn't want any more of it.

Hughes: Were there patients killed?

Reeves: No. Nobody was harmed in the hospital. I believe there were eight people killed in Bakersfield. At a farm equipment store, a DC-8 tractor that was on the display floor went into the basement, and the people down there were all squished. It's no fun being in an earthquake. I've been in a lot of them, but they're never fun. I've been in one in South America, I was in one in Japan, and I was in the Long Beach earthquake in the 1930s that also hit Riverside. I don't want any more.

I was curious not too long ago, and I looked through the guest books that I have from Bakersfield. In a relatively short



period of years we had people who visited the laboratory from twenty-one countries and from sixteen states. It's a Who's Who of arbovirology.

In the summer of 1958 I suddenly started getting letters from all over the world. In one week I got a letter from Dr. T. H. G. Aitken, who was at the Trinidad laboratory of the Rockefeller Foundation doing the entomology work. I got a letter from Drs. Otis and Calista Causey, a couple who headed the laboratory for the Rockefeller Foundation in Belem, Brazil, which is at the mouth of the Amazon River. I got a letter from South Africa from Dr. Robert Kokernot, who was at the Johannesburg laboratory of the Rockefeller Foundation. I got a letter from Harold Trapido, and he was at the Poona laboratory in India of the Rockefeller Foundation. Each of them wrote me separately and said, "I'm going to be on leave in the United States. Would it be possible for me to visit your laboratory?"

So I got them all to come at the same time, and originally they didn't know the other people were coming. I also had Dr. Harald Johnson, who was already here on assignment from the Rockefeller Foundation, so we got all these people from five different Rockefeller Foundation programs together at Bakersfield. We had a great time. They were so pleased to see each other, and we talked about their programs and worked day and night getting around the field to see things and so on.

Then in the following year I suddenly got a telephone call from Dr. Meyer. Dr. Meyer loved to call me and tell me I was going to have a visitor. He said, "Reeves, your Ruskie buddies from Moscow and Leningrad are showing up in Bakersfield." "My buddies?" "Yes. Dr. Chumakov." This is M. P. Chumakov, who became the famous man in Moscow on polio but also had started the studies in Russia on encephalitis. As a matter of fact, he was a victim of Russian spring-summer encephalitis and still is partially crippled. His wife, Marina K. Voroshilova, was with him. She is a scientist and has a different name than his, but they're a married couple and have children. And Anitol A. Smorodintsev from Leningrad was the third visitor and an outstanding scientist.

They were all coming together because they were at a polio conference in Washington, D.C. They were having the first conferences on their field trials of the Sabin polio vaccine in Russia. So they were coming to report on their findings at this meeting in Washington. At the end of the meeting they had suddenly announced to Dr. Joe Smadel, who was heading this review group, that they would come to California to see Reeves. That was news to Joe Smadel, because he couldn't figure out any reason why

these important Russians would want to come to Kern County, California, of all places, and to see me.

So when Dr. Meyer called me, I said, "Dr. Meyer, they can't come to Kern County. It's off limits to Russians." It was one of those times when our government was not allowing Russians to come to Kern County because American visitors couldn't go someplace in Russia.

Hughes: There was really nothing secret in Kern County?

Reeves: Well, there are a lot of oil wells and some air bases and so on. Anyway, I said, "They can't come here. It's against the law." He said, "They're coming." You didn't argue with Dr. Meyer, so I said, "Dr. Meyer, when they arrive, I'm going to assume that the State Department in Washington, D.C., has cleared them, and I'm not going to worry about that anymore." He said, "That's right."

So they showed up. I'd met them earlier, in 1959, at the meetings in Lisbon of the International Congress on Tropical Medicine and Malariology, and we'd become friends. So they decided they ought to come and see what we were doing, and they did.

Hughes: Did they both speak English?

Reeves: Smorodintsev spoke very good English, and Marina spoke very good English. Mike Chumakov supposedly didn't speak any English. However, I never had any trouble conversing with him when the two of us were alone, and I don't speak any Russian. I only know *nyet*.

He and his wife were here later on a visit, and they were staying at the Claremont Hotel. I managed to get them separated from their watchdogs and their associates one night so my wife and I could take them to dinner. We were walking out of the Claremont Hotel, and I'm walking with her. She's wearing a beautiful mink coat, but she's not built to wear a mink coat. [laughs] Anyway, it didn't fit her very well. Mike's walking back with my wife, and it was a very nice evening. He said, "Isn't that a beautiful moon? What a beautiful night to be out together," just like that, you know, without thinking. [laughter] My wife told me about it later, and she doesn't speak Russian. So he does understand English.

So anyway, yes, we had a lot of visitors. I think that's a sign that what you're doing is of interest, when people from all over the world know what you're doing. It's good for your staff and for the accomplishments that they've done individually.

### Disseminating Research Information

Hughes: Did the word spread mainly through publications?

Reeves: I think through publications, and I think the other way that influenced it was activities of the American Committee on Arthropod-Borne Viruses, which was formed as a result of a meeting called by the WHO and the Rockefeller Foundation during the 1959 meetings in Lisbon. That will be a whole separate topic.

Hughes: Yes, I want to talk about that later.

Reeves: That 1959 meeting really brought attention to our program and many contacts internationally. Plus the fact that I wasn't just spending my time in California. I was going to international congresses like the one in Lisbon, a similar congress was held later in Rio de Janeiro, and I was going to Australia on the Murray Valley encephalitis field project and also going out in the Pacific and meeting people there. So it was a two-way street. When I went to the congress in Lisbon, the Rockefeller Foundation had asked me when that meeting was over to make a tour of their laboratory in Trinidad and to write an evaluation. That was my first visit to Trinidad, where Dr. [Wilbur G.] Downs was. I later went to Belem, Brazil, and Cali, Colombia, to see the Rockefeller programs that were there.

Hughes: Were they mainly on yellow fever?

Reeves: No, they were working on yellow fever, but the Rockefeller Foundation also had programs that were scattered worldwide in the United States, Africa, India, South America, and the Caribbean. The original projects had been on yellow fever, but they had broadened out and were headhunting for viruses.

Hughes: But only arthropod-borne?

Reeves: That's right. That was their principal objective. They also isolated new strains of rabies and other viruses. But their real objective was to find as many arboviruses in the world as they could and to add to that collection as fast as they could. So I think to call them headhunters is not derogatory. They did an amazing job with the newest tools as far as methods of isolating viruses and so on and under very difficult conditions. They just went out and isolated viruses and studied the diseases they were associated with. They concentrated their studies in the tropics,



expecting there would be a lot more viruses there than there are in a temperate area.

I did consulting work for the Pan-American Health Organization on a number of occasions. One time they sent Dr. William F. Scherer from Cornell and myself all the way around Latin America to review every arthropod-borne virus program that was there to advise the PAHO of the problems and what funding was needed to make these programs more effective. That tour included all of South America, the Caribbean, the whole area--anyplace there was any virus work going on. So the contacts were wide in both directions.

It was sort of fun to do these things, but it was a lot of work. Anyone who says this consultant stuff is easy has never done it. Because you not only have to go, you have to straddle language barriers, and you have to get a real hold on what's going on in each place in order to do what you're being sent to do. You're meeting new people. There isn't always trust between people.

Hughes: Yet you're supposed to be the authority.

Reeves: Yes, you're supposed to be an authority, sometimes on things you really don't know. [laughs]

I remember one time I was asked to go down to the Queensland Institute of Medical Research to do a program review, and I assumed it was to be on their arbovirus program. I got down there and found out that they didn't intend for me to just review the arbovirus program, I was to review their cancer program, a program on the epidemiology of automobile accidents, taxonomic work on mites, anything they were doing. I couldn't have cared less about some of the projects and didn't know much about the subjects.

Hughes: What did you do?

Reeves: I found out I had an amazing veneer of knowledge in all these areas which had accumulated from my broad contact with colleagues and students in our teaching program at Berkeley. So actually, you find out that you can make yourself an interested audience and can listen to what they're doing and get a feeling of whether or not they really know what they're doing. What the director of the institute wanted was an outside view that wasn't prejudiced. But it was quite a shock to go there thinking you were just going to talk about arboviruses and to people that you really knew well, that you'd actually worked with earlier, and then to find out that you were going to spend most of your time talking to people you had never met before and knew nothing about what they were doing.

I found it a little strange but challenging. The outcome of this and other work I did in Australia was that they made me a fellow of the institute, a nice honor.

### Combining Different Techniques in Field Studies

Hughes: The next topic is how you combined the use of carbon dioxide as an attractant with fluorescent dusts to mark mosquitoes and other techniques in your field studies.

Reeves: I think I mentioned sometime earlier that when I went up to Yakima I found that work had been done in New Jersey indicating that carbon dioxide was attractive to mosquitoes. Up there we used it purely for the purpose of getting more mosquitoes to test for virus.

Then, as time went on in Bakersfield, I began to realize there were other things we could use it and other techniques for. The first of those was in flight-range studies. If we marked the mosquitoes with fluorescent dust, then we could catch them after we turned them loose and see how far they'd gone and how long it took them to do it. Carbon dioxide bait was one method we used. We did a big study in 1961 with Dr. R. P. Dow from CDC.<sup>1</sup> We were trying to demonstrate how effective an intensive program of mosquito control would be in stopping encephalitis transmission. We were working on this with the Kern Mosquito Abatement District and the Public Health Service.

We had an area that got to be as big as sixty square miles, in which we had a large number of inspectors and control operators from the mosquito abatement district searching every bit of water to find and control any mosquito breeding. When they found any mosquito breeding at all, they pounded it with insecticides and put fish in the water if it was a permanent breeding site. In the wintertime they went into places where there were chronic problems and used bulldozers and put in drains. At the bottom of agricultural fields where the farmer had waste water standing, they would put a catchment basin with pumps and pump that water back to the head of the field for reuse. We did everything we could for mosquito control. We didn't trust the inspectors, so we personally followed them each week and made sure they weren't

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<sup>1</sup>R. P. Dow, W. C. Reeves, and R. E. Bellamy, "Dispersal of Female Culex tarsalis into a Larvicided Area," American Journal of Tropical Medicine and Hygiene 14 (1965): 656-670.

missing places. We thought they were doing a pretty darn good job, as we'd go out and in a week couldn't find a single source of larval mosquitoes in the forty- or sixty-square-mile area. There were several study areas. The participation and interest of Art Geib, the manager of the Kern Mosquito Abatement, and his staff was essential in that research.

Our objective in the project was to see if we could stop encephalitis transmission, and we thought that if the Kern district did good control in a forty-or sixty-square-mile area, this certainly ought to be enough. If we made observations in the middle of that area, there shouldn't be any viral activity. But in fact the vectors were still there, and there was still virus activity. So now our problem was, are we missing breeding sites, or are the mosquitoes flying in? This was a serious problem, because we were putting all of our resources into coverage of these areas. The Kern district had spent a lot of money on control.

So we decided we had to investigate again how far mosquitoes flew and how fast. This time we used a new study design. Instead of turning mosquitoes loose in the middle of an area and seeing how far out they went, we marked mosquitoes with different colors and released them a mile outside of the four corners of the sprayed area and then collected all the mosquitoes we could in the middle of the area. So mosquitoes from all four directions could go into the area and be caught.

To our amazement, we found that within a very short time--a few hours or a single day--marked mosquitoes went five miles from the outside of the area to its center and from all four directions. So that answered one question; infiltration was occurring.

Then we were able to examine the marking and other mosquitoes individually, and we found that many were older mosquitoes. The females were not all young, freshly hatched mosquitoes. A very high proportion of them had had a previous blood meal and had laid eggs. We'd had to adopt techniques from work in Russia<sup>1</sup>, in which we took the ovaries out of the female mosquito and examined them. If they had laid eggs, there were several markers on their ovaries that you could see physically. So you could take ovaries out of a female, and you could tell whether she had laid eggs before.

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<sup>1</sup>T. S. Detinova, "Age Grouping Methods in Diptera of Medical Importance," World Health Organization Monograph 42 (1962): 1-216.



We also showed that these mosquitoes were feeding almost entirely on blood from birds. So we realized that we had additional things we could do with the mark-release dust, as we realized that by combining marking of mosquitoes with ovarian development we could learn how old mosquitoes were. We could begin to construct what we call life tables, which means how long the average mosquito lives under certain circumstances. That was a very important bit of information to get, because after a mosquito feeds on virus it takes some time for the virus to multiply in the stomach and to get through the stomach wall into the body cavity and then into the salivary glands. That's the incubation period in the mosquito. We wanted to know how long they were living and what proportion of the mosquito population was living that long, as this determined how effective they'd be as vectors.

At the same time we were using dry ice in these studies. We found that if we used dry ice as a bait we could collect the mosquitoes that were interested in biting. They won't go to dry ice unless they're interested in getting blood. Now, that's different from collecting all the mosquitoes that are out there. If you collect from the total population, there's going to be a large proportion that have just hatched. If a large part of those mosquitoes die before they get older, they won't be vectors. We also had a dilemma because we didn't know what proportion of mosquitoes that we found infected were able to transmit virus by their bites. We just didn't know that. It hadn't occurred to us that this was a problem. We had assumed that any mosquito that was infected was a bad mosquito that would transmit infection.

### A Biological Model of Virus Transmission

Reeves: To make a very long story short, what we found was that by using all of these techniques--carbon dioxide attraction, letting a mosquito bite a chicken to see if the infected mosquitoes could transmit virus by their bite, marking and releasing to see how far mosquitoes went and how old they were when they were recaptured--we could begin to construct, not a mathematical model but a biological model of what had to happen for virus transmission to be effective. We found that on the average only one in four infected mosquitoes could transmit the virus by bite; they weren't all bad. We found that the reason for this was that most of the mosquitoes that fed on a virus source didn't live long enough to be able to transmit. Originally we had thought every infected mosquito was a bad mosquito. I still can't convince some people

in mosquito control that every infected mosquito is not a bad mosquito.

Hughes: How does that information help in control?

Reeves: It gives the control people some time, as it will take some time from the first detection of virus to when mosquitoes can transmit. During this time they can apply insecticides to increase the mortality rate in an adult mosquito population. We find that in Kern County in the summertime 25 to 30 percent of the adult female mosquitoes in a population may die every day. Now, you don't have to be a mathematical genius to see that if that percent of a population is dying every day, that ten days after they hatch there's not a high proportion of those mosquitoes left there. If you started off with a thousand, there'd be maybe five left after ten days. If you put into this model the proportion that had to feed on virus to get infected, and you knew there weren't that many sources of virus out there, you wondered how the virus could survive. As a matter of fact, what you wind up thinking is that these viruses can't possibly be out there, they cannot possibly be persisting, they cannot be successfully transmitted. It's impossible. If you can increase the mortality of adults by use of insecticides, it will decrease the likelihood of transmission further. What you realize before you're through is that there are millions of mosquitoes in the area.

Hughes: So some of them are going to get infected.

Reeves: Some of them are going to carry the virus through.

#### More on Mosquito Control

Hughes: Isn't one reason the mosquito abatement districts are reluctant to limit spraying because they're also interested in decreasing pest mosquitoes?

Reeves: Yes, but they still want to have an economical as well as an effective program. They don't simply go out and use a shotgun approach to kill all of the mosquitoes. If you want to prevent disease, you concentrate on the particular species of mosquito that is the principal vector. If you want to control pests you don't go out and kill all mosquitoes, because some species never bite a person.

Hughes: Could you tell them how to do that?



Reeves: We can identify which mosquito species are transmitting virus and then can say, "This is the mosquito you're interested in." We also do biological field studies, so we can say, "This is where this mosquito is coming from." This allows them to concentrate their control activities. For example, you rarely find *Culex tarsalis* in water that is not sunlit and reasonably clean. So you usually won't find them in water from a septic tank or in shade.

I won't go into further detail, but we can give abatement districts a lot of information if they want to control disease in which mosquitoes are the bad ones or when mosquitoes are pests or where they're coming from. Now, once that book is written, they take the information for granted. Everybody knows it, so it's no secret or new information anymore. The interesting thing about research is that a new discovery is made, and a year later everybody takes this knowledge for granted and don't even remember where it came from.

Hughes: The mosquito abatement districts were taking your information and really applying it?

Reeves: Yes, but I must emphasize not just our information, as many people were involved, including the mosquito control people. The State Health Department and university staffs were deeply involved in development of new knowledge and in convincing the larger mosquito abatement districts that it was absolutely essential that they have a professional entomologist on their staff, not to do research of the type we were doing but to be able to work with their field control people, the operators, on where the problems were, to point them in the right direction, and then to go out and recheck to make sure that they were doing the job they were supposed to be doing.

A number of people who have graduate degrees in entomology, other biological sciences, and engineering are now working full time in mosquito abatement districts and bringing the programs to high levels of performance. As an example, Dr. Richard Meyer, who worked with us for seven or eight years in Kern County, for personal reasons didn't want to be subjected to the hours and travel that he had to work in our program. He's now the full-time entomologist with the Kern Mosquito Abatement District. They've had one or more full-time entomologists in that district ever since Mr. Geib took over there in 1946.

Hughes: Is that unusual?

Reeves: Employment of an entomologist is not unusual, as there are many other districts that have done the same thing. These are primarily the larger districts that have the largest budgets. You



have to realize that in California today mosquito control is a \$60 million industry. One of the larger districts had a budget of over \$4 million, and seventeen other districts had over \$1 million budgets this past year for mosquito control. That's not peanuts. I could do a lot of research for that. [laughter] It's a big business now. There are sixty-three organized mosquito abatement districts in California. In recent times we've had regions like San Diego and San Bernardino counties develop countywide districts that were just small activities before then. It's in response to an increase in population and an increased demand for protection from mosquito bites, both as pests and as disease vectors.

Dr. William K. Reisen, who is the head of our field program, has been utilizing all the research methodologies I have been referring to. He can now go into an area and in a relatively short period of time can do a study on the life table of the mosquitoes, show how long they live and how far they fly, and use all of these techniques rapidly and very efficiently. He's been doing this currently in cooperative studies in the Los Angeles metropolitan area, the Coachella-Imperial Valley, and in Kern County. He just finished mark-release studies of *Culex* mosquitoes in Kern County that was reported at the mosquito control conference. The objective was to see if mosquitoes were following the Kern River waterway as a flight path and going five miles or more into urban areas. He used all of the techniques that we developed as well as techniques that he'd learned while working internationally in Guam, Southeast Asia, and so on.

#### The Staff at the Bakersfield Field Station##

Reeves: As I said earlier, in 1945 we decided that we could no longer have just a summer program in Kern County, that we could not really study the activities of the mosquitoes or the viruses until we encompassed the entire year. That meant we had to have a permanent staff in residence in Kern County. It wasn't enough to run down there from Berkeley or San Francisco every couple of weeks, do a few observations, and leave. You had to be living there with the problems. You had to experience them in rather miserable times like the middle of the winter in Kern County. At such times you may not be able to see your hand in front of your face because of the fog, and the water may be frozen, but the mosquitoes are still there.

So in 1946 Dr. Brookman from the Public Health Service moved to Kern County, and he stayed there until about 1950 or so, when he wanted to move. He thought he should broaden his experience.

He'd finished his Ph.D. thesis with us on the biology of *Culex tarsalis*. So in 1950 he went to the CDC research station in Greeley, Colorado, where they were doing other virus work. At that time CDC assigned, with my agreement, Dr. R. E. (Buck) Bellamy to take Brookman's place. I had not met Bellamy before, but I knew from his work on *Anopheles* mosquitoes in Georgia and other parts of the southeastern United States that he was a good, well-trained entomologist.

So they moved him to Bakersfield in 1951, and I put him in charge of the field station. He stayed there until 1967, and that's a long-time assignment for the Public Health Service in one place. As a matter of fact, a number of times they wanted to move him elsewhere to do different things, and I said, "Look, just leave it the way it is. It's going very well. He's a very competent entomologist and very interested in minute details, which we need." I mean, he wrote down biological observations that you thought at the time were nonsense and that turned out later to be very important.

So Buck stayed with us for that long period of time, until he decided that it was time to retire from the Public Health Service. He then went up to Canada and worked there at a research laboratory at Belleville in Ontario, Canada. When Buck left the CDC they no longer were willing to assign people from CDC. They'd been doing it for twenty-one years, from 1946 to 1967, and they now had their own encephalitis programs. They also were having budgetary problems and were short of money for personnel. It didn't stop our relationships with the CDC, but they stopped assigning people.

I then sent Dr. Richard Lyness to Bakersfield to be the director. He had just finished a Ph.D. with me here in epidemiology and virology. He was the director until April 1971, when he took a job as the manager of the Tulare Mosquito Abatement District. Again, it was because of family and political reasons that he wanted to get out of the position he was in. Mr. James Bruen was director until 1974.

I had Robert Nelson on the staff, assigned to a subproject in Chico from 1969-1974. He was a very competent entomologist and had been there since 1963, assigned from the State Health Department as a cooperative endeavor with us. We put him in charge of the Kern County program in 1975, and he stayed there until 1980. Again, he was a fantastic person. He was an excellent entomologist, he knew how to handle people, and he worked well with the people in Berkeley. The trouble always was that a person down there in charge of the field station had to be a pretty independent operator day by day, but he also had to keep

in contact with headquarters and handle a varied staff in the field. The staff one year might be fifteen people, and the next year might be five people, so he had to manage a variety of people--students, professionals, visitors, et cetera.

Hughes: Did these heads of the laboratory do a good administrative job?

Reeves: Yes, amazingly, because none of them had actually been trained in administration. It was not a big administrative job, but it still required handling people and getting the jobs done. It also meant that you had to be the sort of person who wasn't constantly looking at your watch.

Hughes: Did you have a part in appointing these people?

Reeves: I chose them.

Hughes: You were looking for administrative ability as well as scientific ability?

Reeves: I looked for a person who was willing to work endless hours, on his own, and on really difficult problems. However, I didn't leave them alone, as I spent every summer in Kern County from 1943 to 1967. I spent the whole summer there unless I was off on some other project. Until I took over as the dean of our School of Public Health in 1967, I was there every summer, working in the field. Not directing; I was down there working and left direction to the field director.

Hughes: What did your family do?

Reeves: Lots of the summers they left San Francisco or Walnut Creek came down to Bakersfield. We had an apartment in San Francisco until 1949, and my wife would pack up the one or two children we had at that time and come down there for the summer. Later, after we got a house in Walnut Creek, we would exchange houses with a schoolteacher down there who wanted to go to summer school at Berkeley.

In 1949 my wife came down and informed me on arrival that she had just put all of our furniture into storage, and we weren't going to live in San Francisco anymore. An apartment in San Francisco was hard to get; and ours was within two blocks of the medical school, which was convenient to me. She said I was spending most of my time in Berkeley anyway. She was a hot-weather girl from Riverside, California, so she wanted to move someplace where it was hot in the summer. I said, "What are you going to do?" She said, "I'm going to buy a house." I said, "How



are you going to do that? We don't have any money." She said, "My father is going to finance it."

Hughes: You couldn't argue with that.

Reeves: I said, "I can't go help find a house. I have to stay here." She said, "I'll do it." So she and her parents came up and were gone for about a week. She came back and said, "I bought a house." I said, "Oh? Where?" "Walnut Creek." I said, "Why not in Berkeley?" "Too expensive. Too big." She bought a house in a brand-new development, sitting on a bare lot in Walnut Creek.

Hughes: And that's where you still are?

Reeves: We're still living there and agree that we have no interest in moving.

Hughes: I'm sure it's not a bare lot anymore.

Reeves: No, it's been pretty well plowed up, fertilized, and planted, with three greenhouses for my orchids.

But anyway, Nelson was in charge in Bakersfield until 1980. Now, that's a long time for a person to be in charge of a field station like ours and participating in a wide range of research activities. He did a lot of very, very, good work. He trained a lot of people, and a lot of things got done. When he left, I was very fortunate in being able to recruit Dr. William Reisen, who is there now. We did an international search by advertisements and found him in Pakistan. Bill's now been in Bakersfield for eleven years, and he's an unusually competent entomologist who is always interested in learning new things. He was perfectly willing to learn how to do virology work. Dr. Hardy goes down and trains these people at the field laboratory how to infect mosquitoes and how to do vector competence and transmission studies. Bill Reisen is perfectly willing to bleed chickens or do any other aspect of the research that many people would want to delegate to a helper.

Hughes: Were all the laboratory heads entomologists?

Reeves: Yes, they've all been entomologists. The reason for that is that the focus of our program is entomological, while it isn't limited to that. If you're looking for cases, you have to have a physician or a medical student ready to do that. So that's a seasonal sort of person. If you want a veterinarian to chase cases in horses, it's also seasonal.

Currently we're studying the overwintering of viruses; we're studying what a vector does during the wintertime when it's not

actively reproducing and producing large populations. Until 1971 we had a vertebrate zoologist there almost all the time. Dr. Donald Roberts was the last one. It was a year-round study to determine when the birds were there, which times they were gone, what are the different species, whether they have antibodies or not. The zoologists have worked very well under the entomologist, but that didn't mean that the entomologist had competence in vertebrate zoology. It meant that he was able to direct them. Frankly, it's been necessary, because sometimes vertebrate zoologists have a tendency to get buried in studies on the biology of a vertebrate that isn't really the basic problem that concerns us. Knowledge of which side of a limb a bird hangs its tail over isn't necessarily going to help us understand virus transmission. I think I referred earlier to the problem of McClure at one time, who couldn't shoot or catch a dove. They have their idiosyncracies, as we all do. But if you have a person directing the research who understands the overall objective of the program, it works out. Don Roberts carried out year-round studies on small rodents in a large study area on the west side of Kern County.

Hughes: Did you feel it was your responsibility to be the ultimate overseer of these field programs?

Reeves: I don't know what you are getting at, but somebody always has to be at the end of the broom and understand that the buck stops there. Actually, I think the responsibility of a person in the position that I was in, or Hammon was in earlier, was that you're the person who has to shake the apple tree and get the apples down; apples are the money that you need to do the research. You have to make sure that your program isn't getting into dead ends and just stopping there and that it is innovative. So if you were to look at our progress reports, you would see that our program has continually moved into new areas and new methodologies.

Hughes: Was that largely your doing?

Reeves: Yes, but it was not a solo effort. You have a lot of communication about such changes with everybody in the program. I'm no longer the head or end of the line on this. Dr. Hardy is, but I still communicate my ideas, and some he likes and some he doesn't.

### Funding the Research Program

Hughes: What kind of balance did you try to strike between what you felt was necessary to do from a scientific and a practical standpoint and the money that you thought you could get?

Reeves: Our "consumers" were varied. The mosquito control districts wanted to do a better job of controlling the vector. The medical people wanted to know what were the diagnostic problems, what they could contribute on the diagnostic or prevention end. The people at CDC or the U.S. Army or the National Institutes of Health granting sources wanted to have original research that was on the cutting edge of new knowledge. They were not particularly interested in applied research.

We were fortunate in some ways, because sometimes we could tap into a source of funding for applied research with the state program, where the money was coming for a fairly directed objective. I mean, how do we control this, how do we do this, how do we do that? They didn't want "pie-in-the-sky" research.

The army might be interested in some specific question that they were willing to finance. Vector competence for virus diseases happened to be one of those. Another was how to do a better job of aerosol control of adult mosquitoes. So we could go to such agencies with specific problems. The army was interested at one time in financing the mosquito blood meal identification work, because this approach had broad applications all over the world whenever they were studying vectors that were bloodsuckers; they wanted to know what the vectors were feeding on.

We were able to convince NIH that having the multifaceted sort of program we had would provide an overall epidemiological view of vector-borne diseases applicable in any area of the world. We have been able up to now to sell them on that. They frequently have come to us and said, "Why don't you divide this research into six separate projects?" I have always said, "Because if there are six separate projects they won't fly. We'll get off into six directions that will get isolated. We want all our projects to interact with the objective of gaining a broad ecological knowledge that will lead to better control of arthropod-borne viruses." We've been fortunate in being able to sell them a big bill for this. I mean, I'm talking about getting hundreds of thousands of dollars per year. I'm not sure how long we can continue with this as there is competition from newly emerging diseases such as AIDS and a decreasing occurrence of encephalitis.



Hughes: The reason they were hoping to divide up the research was to cut the costs?

Reeves: In part. Because if you cut it up, then they can approve one project and not another one. But I see an interface between projects that I'll fight for, bleed for, and die to keep together. The overall evaluation of the program I guess was such that they've paid the price of letting us do some of the things that they weren't particularly excited about.

One time early in the game I went to NIH with a project for blood meal identification as a separate project. I thought it was a good project, and it went through the study section. The first time that I knew something was wrong was when I got a call from Jerry Syverton at Minnesota, who was on the NIH Advisory Council for the Allergy and Infectious Disease Program. In research grant considerations there are several tiers in the Public Health Service. There's the study section tier, which does the preliminary scientific reviews and ratings, and then all projects go up to the scientific advisory council for an institute, which is advisory to the director of NIH or whatever subsection it might be, for final grading and deciding whether it's going to get funded or not.

Jerry Syverton was not a person I knew very well. However, I knew him because he was on Guam in 1945 in the navy unit in Okinawa when I was there for the Jap B encephalitis investigation in 1945. He was a very competent physician and virologist. Anyway, Jerry was on the council, and my research grant application for blood meal identification had come up and didn't have a high enough priority to be paid. He called me and said, "This is a good project, isn't it?" I said I thought so. He said, "Well, I hope so, because I told them if Bill Reeves wants to do it, by God it's going to be funded." And it was.

Hughes: It was simple as that.

Reeves: Well, that was an unusual development. Number one, it was unusual for him to tell me; and number two, it was unusual for it to be funded. It did turn out to be a good project, and for a time we built a better mousetrap for mosquito blood meal identification that everybody wanted.

Anyway, so the funding of research was not easy, and we also found that you'd better have multiple sources of funding so that if something did fall through, you always had something going.

Hughes: So what, in summary, were the major sources?

Reeves: CDC was giving us people and an operating budget because they had people here. The National Foundation for Infantile Paralysis supported us in the forties because they wanted epidemiological studies on polio to be done in this area, and they also wanted a further differentiation of the epidemiology and the diagnosis of encephalitis and polio. The U.S. Army wanted evaluations done on DDT. They also wanted evaluations done of another aspect that was particularly of concern to them, namely, the epidemiology and control of mosquito-borne diseases. NIH was concerned because they were interested in the overall picture of arboviruses. State Fish and Game was interested in supporting us with people and money because they were interested in the bird malaria project. This was a disease of birds that they didn't have the facilities or the staff to do studies of, so they found it was of mutual interest. The local mosquito abatement district gave us a lot of support. They gave us personnel support; they even gave us money for various things, like picking up the cost of automobiles or feeding sentinel chickens. These activities were of interest to them. Now, that might not be cash that you had in a research grant sense, but it took away the need to get research grant money for that kind of thing. The State Health Department provided money to obtain answers to specific problems related to vector control.

We took every avenue we could to get support for any aspect of the program that we felt strongly should be done. But we very rarely took money that was initiated by somebody else who had a bright idea they wanted done their way. The latter is not a good way to do good research. We have had plenty of opportunities in today's world to make a contract with an outfit, and we could get money to do things, but they'd be things that somebody else thinks are great ideas and something they want done. We have told the army that we won't do research on some projects because it's not what we're here for.

Now, some of the refusals to do certain types of research are self-preservation in the sense that if you did what was wanted, it wouldn't be high class research. People on the staff need credit for good research for promotions and are interested in the stability of their positions. They would be dead ducks if you agreed to do certain contract research. That's putting it pretty bluntly, but it's publish or perish in this business, whether you like it or not, and the research better be good. The one thing that's fortunate about the university that is different from some governmental agencies is that in the university you don't necessarily have to go up, up, up, up to where you're a top administrator and can no longer do research. You can stay in place, be a professor, and do nothing but research and teaching.



So you have to guard yourself and your staff from outside influences.

Hughes: I would gather that these different funding organizations have different goals. [tape interruption]

Reeves: Your question is whether each of these funding agencies has its own objective? Yes. If they had their choice, I think representatives of most mosquito control districts would say, "We want all of the effort at the university to go into those particular aspects that are of direct concern to us." They would say, "Our jobs out here are on the line; we're on the front line. This is the information we need."

With that argument, you also may not get money from the National Institutes of Health. As a matter of fact, we've had things that have been cut out of project proposals. They have said, "That's a California problem. It has no application outside of California." I always say that they're wrong, because I think they are wrong. We're not doing this just for California. But they can have their limits, and they're interested in pure research. They're looking for cutting edge stuff. Indeed, sometimes I get very worried that they will have a group review of our proposals that is all by bench molecular biologists. It's awfully hard to convince them that it makes any difference where a bird flies or how old a mosquito is, that you must spend large sums on travel to do field work. That's not a criticism. In return you can't convince me as easily as you can one of them that what they're doing in molecular biology has any future. As a matter of fact, I'm a very difficult reviewer for a molecular biology project.

The CDC in large part has been a state service organization, and they have their objectives, and the army has theirs. (The problem is to find a fit of your research interest with theirs.) But yes, they may still dictate to some degree what you do or not fund your research.

### Keeping the Research on Track

Hughes: Were you the one who tailored the grant proposal to the appropriate funding agency?

Reeves: No. Again, it's a group effort. I think that somebody always has to be responsible and say, "Look, this won't fly, this will fly," as far as funding is concerned. That is difficult, because



sometimes the people that are isolated in the field or laboratory are not thinking that way. They're thinking of what's most exciting and interesting to them individually. The people in the laboratory discover a new virus, and all they may want to do is work on that new virus. It's new, yet it may not be worth five cents as far research money is concerned. You may have to say, "No, we can't do that."

In the case of California encephalitis virus, that's exactly what we did. We put it in the Revco freezer for safekeeping when we could only identify three human cases of that disease and knew it wasn't going to be a veterinary problem. We dug the virus out a few years ago when other viruses in that group became major public health problems in other areas of the United States and there was more of an interest in such viruses.

Hughes: How could you be sure it wasn't a public health problem?

Reeves: We were looking at every sick person and horse we could find in Kern County where the virus was. We found a lot of people were being infected--that is, had antibodies--but had no associated illness. Mosquitoes weren't being careful who or what they infected, but the people or animals didn't seem to have a disease to go with their infection.

We have had research staff who got off on their little tangents, and there was no shaking them off it. In such instances we said it shouldn't be pursued, and they said they were going to and did. In such cases you come to the end of the road when it threatens funding. I won't name people, but I could, who just had to move because they wouldn't work on the basic problem that was the theme of the project. What you can do that gives you more leeway is to have graduate students take up peripheral topics as a thesis problem. In this instance it can be complementary to the overall program, but it does not represent the guts of the overall program.

Hughes: And you encouraged that?

Reeves: We encouraged that. As a matter of fact, we encouraged it very strongly. It gets a lot of things done that you wouldn't ordinarily get done, and it's done cheaper that way. But you also don't put the central part of the research program out there for them. (For the major projects you better get a good technician or yourself on the problem and get it done.) You can't give every problem away to somebody else. As far as I'm concerned, once a student has undertaken a problem it's his or hers and not mine anymore, even if I'm chairman of the committee.

Hughes: Were you successful in keeping the program on the track that you wanted it to be on?

Reeves: Yes, generally I think so. You begin to see an evolution. We started with a very specific objective, and it was to learn if mosquitoes were vectors [of encephalitis]. Now they're vectors. Then we wanted to learn what additional things needed to be known about mosquitoes if we were to have control. Then we went to the problem of overwintering of the viruses; how does that happen? Some of the projects evolved from where we started, but now we are a long way from where we started and are into the factors in a mosquito that control vector competence, the influence of global warming in mosquito-borne diseases, and viruses in mountain and salt marsh mosquitoes.

Hughes: When you changed tack, for example getting into mosquito genetics, was it because you felt that that really was the direction the project should go, or was it because genetics was where the money was?

Reeves: No, that's not where the money was. We had to sell that program to get the money.

Hughes: Did I pick a bad example?

Reeves: No, it's not a bad example, because some people were pushing genetics as the answer to all insect control programs as it didn't require the use of insecticides, so there was a lot of publicity, and there was an opening there. However, before we hired a full-time Ph.D. geneticist to go into this problem and redirected field research to support it, I had to make the decision that this project had a real potential opportunity to give us a new approach to control and that somebody else wasn't going to do that research before or for us.

### The 1952 Epidemic

[Interview 6: February 12, 1991]###

Reeves: By 1952 we thought we had made tremendous strides forward in the control of both western and St. Louis encephalitis by controlling *Culex tarsalis*. One of the primary reasons for our optimism was that we now had DDT, with which we'd done the first work in 1945, and it seemed to be the cure-all for all problems in mosquito control.

In the winter of '51-52 we had very, very high snowfall in the Sierra Nevadas and very high rainfall in the valley. It almost broke all records for water availability in the state--the sort of thing we could use now in 1991 [the fifth year of drought]. The result was a tremendous flood in the spring when the snow started melting and after the ground had soaked up all it could. We didn't have the big chain of flood control dams and water conservation dams that we have now. In 1952 there were no flood control dams on the Kern or other San Joaquin Valley rivers. Most rivers had no major dams, and those on the Sacramento River and other northern rivers had just been or were being built. The first water from Shasta Dam was delivered in 1951.

So when that snow melted, the water came downhill. When it came downhill from the Sierra Nevadas, the only place it could go in 1952 on the west slope was into the Central Valley and then through the Golden Gate to the Pacific. The result was, the Central Valley literally went under water. The rivers all went over their banks, over the tops of the levees, all up and down the Central Valley. By May we knew we had water everywhere, and *Culex tarsalis* had gone through a couple of springtime breeding cycles. The overwintering females had come out and laid their eggs, and they'd hatched and taken more blood meals; so we'd gone through several generations by May. *Culex tarsalis* has an ability to develop a large population quite rapidly in a warm early spring period. So we were seeing *Culex tarsalis* populations of a size that we hadn't seen before.

Hughes: Why hadn't DDT taken care of them?

#### Mosquito Resistance to DDT

Reeves: You ask, why hadn't DDT taken care of them? Well, number one, there was so much water that it was everywhere, and anyplace there was water standing on the surface, there were *Culex tarsalis*. We didn't have the airplane ability to distribute DDT that widely, and we didn't have the aerosol generation ability to do it from the ground. We thought those were the basic, primary problems we had, but then we began to realize that even where they had sprayed, control wasn't being successful. For the first time, it was recognized *Culex tarsalis* and other mosquitoes had been exposed to enough DDT that genetic selection had taken place, and we had mosquitoes that were resistant to DDT.

Hughes: That was the first example of insect resistance?



Reeves: No, it wasn't the first example of this. It had been recognized some in agriculture. But the assumption in mosquito control was that they had not been applying DDT that widely and not so much of it that it would have forced genetic resistance to take place. As a matter of fact, genetic resistance just wasn't something that was on most people's minds in any aspect of insect control at that stage. The only thing I knew was that some of the scale insects on orange trees in southern California were resistant to cyanide. But really, the idea that this miracle insecticide, DDT, was not going to be effective because of genetic resistance was a brand-new thing.

Hughes: But antibiotic resistance was a well-established concept in medicine, wasn't it?

Reeves: No. Antibiotic resistance also was just coming to the surface. You have to realize that we had no antibiotics before 1946, none. Penicillin was not being taken by people before then. Streptomycin wasn't being taken. None of these antibiotics were on the market. They were new, they were experimental until the post-World War II period. So we had not had those experiences that accumulated very, very rapidly in the post-war period. If you told a physician in 1952, "You ought to be careful; you're going to have penicillin resistance," he wouldn't have known what you were talking about.

So we began to have failures in control and, of course, when you began to have failures you always blamed the most obvious things: the insecticide wasn't applied correctly; the wrong dosage was used. Or the person who said, "I sprayed this" never did do it; he was goofing off, sitting under a tree drinking a cup of coffee and smoking a cigarette. So we always assumed that there was something wrong with the application, with the equipment, or with the material. It wasn't until after the epidemic had really erupted or was over that the whole story of insecticide resistance began to come out and was appreciated for what it was.

Well, anyway, they were valiantly out spraying DDT anyplace that there was a control agency set up to do so. Now, we were pretty confused. We thought that we had the dog by the tail in Kern County. Do you get bears or dogs by the tail? [laughter] I don't remember.

Hughes: I'm not sure.

### Human Cases

Reeves: Anyway, we had a good mosquito abatement district, and we had an excellent research staff in Kern County. Here we were, sitting with what you might say was the world's research center on these viruses. To make a long story short, by the end of that summer we'd had a hundred proven cases of western equine encephalitis and sixteen cases of St. Louis encephalitis in Kern General Hospital-- and this was more than twice the number we'd seen in any year before. So both viruses were very active; we had proven clinical cases of WEE and SLE, we had deaths, we had a major epidemic. We still haven't surpassed that number of cases of western encephalitis anytime in our experience in Kern County as far as human cases are concerned.

The epidemic wasn't limited to Kern County; it was throughout the Central Valley, all the way from Lassen County in the north to Kern County in the south. The only area of the state that really escaped that year was southern California. The metropolitan area of L.A. had no cases. There were a few cases scattered around in Riverside, San Bernardino, and Imperial counties. It really was a major disease, and we wound up with 420 cases in the state that were proven to be western or St. Louis encephalitis. I keep stressing "proven," because many additional cases never could be proven; the right specimens didn't come into the laboratory so it could do a good diagnostic test, plus the fact that the state laboratory couldn't possibly have anticipated the epidemic. They had to find additional resources in order to do the tests on this flood of material, and they did a wonderful, really excellent job.

Hughes: So there probably were many more cases than four hundred?

Reeves: Oh, we know there were. I would estimate there could have been a thousand cases. The problem was that when you got a case in, you might find antibodies in the first blood sample that was submitted by the physician, and then if you didn't get a second blood sample you couldn't show a rise in antibody titer, or maybe the first sample was taken too late. There were many cases that were clinically typical and for which you would expect to get a diagnosis of western equine or St. Louis encephalitis. However, we didn't have the modern techniques to differentiate the various types of globulins that are present early and late in the illness. We could do a much better job diagnosing such cases now.

Hughes: So cases such as those would not be classified as encephalitis?

Reeves: They were diagnosed clinically as encephalitis, but they could not be reported as western equine or St. Louis encephalitis in the absence of a specific laboratory diagnosis.

Hughes: Oh, I see.

Reeves: So there was a broad category of cases that were reported as encephalitis, aseptic meningitis, or central nervous system disease. That's how we know we probably had close to 1,000 cases in the state but could only prove the 420.

Now, the good thing about the situation was that the state laboratory really was prepared and could do the testing they did. When Dr. Lennette took over the Viral and Rickettsial Disease Laboratory in 1946, he took over the whole statewide diagnostic service for these and other viruses. They were set up to do the right tests that were available at that time and to do them correctly. It was a matter of getting more personnel in and expanding their operation, which fortunately they were able to do. They had good staff in all aspects of study at the State Health Department, whether it was on the epidemiological, laboratory, or vector control aspects. They knew what they were doing, but they couldn't anticipate DDT resistance. When you get flooding of the type we had, you can't control the water, because it's everywhere. You can't go out and pump it to put it someplace else; there's no place for it to go, and there weren't adequate dams for its storage. So the water sat there in the Valley all summer producing mosquitoes.

We learned a number of very valuable lessons from the 1952 epidemic, and it also got a tremendous amount of attention from the state legislature and the governor's office. A lot of supplementary money was given for emergency control measures. Unfortunately, those emergency control measures, whether they were to kill larval mosquitoes in the water or adult mosquitoes on the fly, were not very successful.

So this brought very strongly to our attention the fact that we would have been better off if we could have anticipated this epidemic. The earlier findings had shown the time lapses between virus appearing in the mosquitoes, followed by the appearance of disease in horses, followed by cases in humans. All of this happened when summer temperatures were hot enough for a virus to be incubated in the vector and transmitted. We had established the sequence of events, and that's when we began to think about developing a real surveillance system for the state that might anticipate that an encephalitis epidemic of this magnitude would take place.



Hughes: Yet given DDT resistance, if you had been able to predict the 1952 epidemic, what tools would you have had to fight it?

Reeves: We wouldn't have had an awful lot of approaches at that particular time, because we didn't have the array of alternative insecticides now available to us.

That epidemic also brought to our attention that some of the leveeing on some of the river systems in California was not adequate to handle the excess of water that was causing flooding. This finally led to the development of flood control dams in many areas of the state so that water could be held in dams and released throughout the summer or in the fall instead of all at one time in the spring, causing a great big flood.

When extreme flooding of that type takes place, it also paralyzes agriculture, because you can't plant a crop in a flooded field unless it is rice. It also ruins orchards; if an orchard gets flooded for any period of time, it will kill the trees. So there was a lot of economic pressure from agricultural interests to control water better in California in order to escape damage from flooding. Fortunately, in this instance the interests of farming, the interest of mosquito control, and the interest of public health could be satisfied if all agreed that flooding of the type that occurred in 1952 was something to be avoided if possible. So they joined forces in support of the construction of flood control dams and development of water distribution systems of the type that we now have.

### Lobbying in Sacramento

Hughes: Did you speak to the legislature on this point?

Reeves: I can't remember back to 1952 that well. I don't think we really needed to do that much talking, because the headlines were in every newspaper in the Central Valley about how important the problem was. I don't remember that we did anything particularly in 1952 in the way of a lobbying effort to develop these types of water control, but you can be sure the members of the California Mosquito Control Association were letting their legislative representatives know.

We had done lobbying in the mid-1940s to impress upon the legislature that encephalitis could be an important disease. It was 1945-46 when we got the first money for encephalitis control. But here it was, 1952, only six years later. Now, I grant the

fact that we got some flack. People at the Sacramento level said, "We gave you money, you've improved mosquito control in the state, you have miracle insecticides to work with--why are we having this epidemic?"

I think it must have been about that time that I did go to a legislative hearing to explain what had happened. I explained about birds being the source of virus, how mosquitoes got it and so on, and how the infection showed up first in the southern part of the Central Valley and then progressed northward as the season advanced, so that the cases appeared in the Sacramento Valley a number of weeks later than in the San Joaquin Valley.

At that time, one of the legislators did his own thinking and said, "I'm going to pass a new law. I'm going to make it against the law for birds to move from southern California to northern California." At that moment I had a terrible time. I mean, this was a public legislative hearing, and I'm up there on the stand. This guy had solved all the problems just like that. [snaps fingers; laughter] I tried to impress upon him that it wasn't going to be very easy to get the birds to obey that law, that they were going to fly where they wanted to when it was the right time to do it. And then his colleagues began to abuse him a little bit about his denseness. He fortunately had a sense of humor; he saw the point.

When we came out of that summer, we felt that we'd really learned some lessons the hard way. It wasn't enough to think that you knew the answers, to think you have developed control approaches and know what mosquitoes and birds are carrying the viruses. For instance, starting in May, 1952, we could collect mosquitoes anyplace we wanted to and take them to the virus lab and isolate virus from them. Almost every pool of fifty mosquitoes was positive for western virus.

Hughes: Was it mainly western?

Reeves: It was mainly western in the beginning of the summer. By the time we got to middle to late summer, a lot of St. Louis virus was there. It never got as high as it did for western, but it did that just a couple of years later. Two years later we had more St. Louis than western cases, but it wasn't as great an outbreak as in 1952.

Typically in those days, the *Culex tarsalis* population peaked in May, June, or early July. Later in the summer the population started to drop off. The reason for that was that flooding took place early each year. Also, in those days they had a lot more



early season irrigation of crops than they do now. Now the irrigation is more spread out throughout the summer.

Hughes: Getting back to the resistance problem, were the insecticide-producing companies leaping onto the bandwagon and doing their best to come up with new insecticides?

Reeves: Yes. DDT is what we called a chlorinated hydrocarbon. There's a general family of substances that are in that group. The first alternative they came up with was chlorodane. Chlorodane was used to replace DDT and was believed to be the immediate answer. For a couple of years chlorodane worked like a charm, and then insects became resistant to it. The insect control people went through the whole family of chlorinated hydrocarbons to the point that many insects were resistant to anything in that complex.

Now, I don't want to blame the DDT resistance solely on the mosquito control agencies. The amount of DDT they were putting out was miniscule compared to what the agricultural growers were applying. In an intense farming area like the Central Valley of California, insecticides were used as the answer to almost every insect pest problem they had, plus the fact that every household was using them for fly and/or clothes moth control. A mosquito that was living in this environment, even in an area where there was no mosquito control going on, was still being exposed very highly to DDT in its environment. So the selection for insecticide resistance was taking place regardless of whether it was being used for mosquito control or not. This is still true today with the more modern insecticides which have come in after DDT, the organophosphorus compounds--malathion, parathion, that whole family of agents. Again, the agricultural use far outweighed the amount used for mosquito control.

#### The Use of DDT in the Yakima Valley

Reeves: In 1948 we got curious about what was going on in the Yakima Valley of Washington, because we hadn't had any more encephalitis up there. As you remember, we had a series of epidemics up there earlier and learned most of what we know about these viruses, but then we had no more cases. So we went back up there in 1949 to see what was happening. We put a crew together. McClure was the ornithologist, and Alan Longshore and Bill Hammon did the epidemiology work. Brookman and I did the entomology. We collaborated with Dr. A. S. Lazarus from the University of Washington, who went with us and did some of the lab work.



The first thing that we heard, even before we went to Yakima, was that the health officer, M. Stanley Benner, was offering a dollar for every fly anyone could bring into his office. I took this as sort of a challenge and thought I might get rich quick. As a matter of fact, I thought of taking a few flies with me when I went up, just in case I needed them. Anyway, I went in to see the health officer, whom I knew very well. The minute I walked in the door, he said, "Bill, that reward doesn't apply to you. [laughter] You don't get a dollar a fly."

Indeed, it was very, very difficult to find a fly or a mosquito in the Yakima Valley in 1949. It was almost impossible. We went out to all my old familiar collecting spots--the pastures, orchards, and swamps. I knew them like the palm of my hand. They hadn't changed; the water was still there. You could go out there with a dipper and spend hours and never find a mosquito, and this was in places where I used to get a hundred or more per dip.

Hughes: What about other insects?

Reeves: There was hardly anything there. You could go into the dirtiest dairy in the county, and there were no flies.

Hughes: What did the entomologist think about all that?

Reeves: There was no mosquito control going on in the area. They had no mosquito control whatsoever. But when I got into the records in that county, I found there was something like a million pounds of DDT being put out per year in that little valley. Previously they had used various other insecticides for controlling the codling moth on apples; they had been using lead arsenate, not exactly something you want in your environment. They had been using a whole variety of different insecticides for years, and then DDT came on the scene. Earlier, you'd go out and the apple trees would be covered with a white coating that was lead arsenate or whatever they were using for insect control. Now it was DDT. There was DDT everywhere, and there were very few mosquitoes and no virus activity.

That's when I wrote a paper: "Yakima, Washington, controls mosquitoes and flies at no cost--why can't we?"<sup>1</sup> An amazing mosquito control program, and nobody organized it. And no disease or ill effects in man or animals whatsoever was associated at that time with this widespread use of insecticides. Our ornithologists

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<sup>1</sup>Proceedings and papers, 18th annual conference, California Mosquito Control Association 1950, 182:13-15.

had no trouble seeing and collecting plenty of birds in the environment.

Hughes: How can that be? There's a balance in nature; if you wipe out one whole population, aren't there repercussions?

Reeves: Not that anybody recognized. However, I'm not saying that I'm all for what was done. We learned a lot from it in the sense that it was possible for this disease to be controlled. [laughter] Well, it was a terrible experiment, because we had no controls (unsprayed areas), but it was very impressive for an entomologist to go in there. I'll confess I was very narrow-minded in my approach. I wasn't worrying about what had happened to beneficial insects or wildlife; that wasn't my field of effort nor my interest. I thought my job was to find out what had happened to mosquitoes.

Hughes: Was anybody worried about killing beneficial insects? Was there any talk in the press?

Reeves: Nobody was worried.

If anybody in the Yakima Valley had made a lot of noise about this, they wouldn't have been there very long. They would have been out of the county, they would have been out of the valley, maybe feet first and maybe running fast. The agricultural economy concerns would have overridden such concerns. Rachel Carson was maybe still a little girl. I don't know how old she was at that stage.

Hughes: She hadn't written anything.

Reeves: She hadn't written any of her stuff. If so, it wouldn't have been a best seller in the Yakima Valley.

Hughes: Hardly.

Reeves: Undoubtedly people will say I'm stupid, if they ever read this, to have had the attitude that I had, but when all of society is dependent upon something like DDT, or think they are, it's very difficult to get a reversal started in its use, no matter how you feel.

### The Encephalitis Surveillance Program

Reeves: To come back to California: the 1952 epidemic certainly impressed us with its importance and the need to gain more knowledge about what factors control the activity of viruses. There was a need to develop a surveillance system that would be effective in detecting epidemics. So the epidemic had a lot to do with the beginning of an encephalitis surveillance program.

Hughes: Under whose aegis was the program?

Reeves: Who was supporting it at this stage?

Hughes: Well, yes.

Reeves: Nobody was supporting it specifically for that purpose except by volunteer participation. After all, the State Health Department had a budget to fund their laboratory and epidemiological studies. At that particular time they had adequate money to do the diagnostic tests they needed to. When they had an epidemic of encephalitis, they just shifted some people over to that problem, or they got some more summertime employees--whatever was necessary to do the job.

By that date we were being supported rather well by the National Institutes of Health to unravel further the epidemiology of these diseases and to evaluate alternative control efforts. Periodically we would get supplementary money through state legislative action to do demonstrations on encephalitis control. We weren't really ready to go into a full-scale state surveillance or control system, but what we started doing was to supplement the existing ways of getting information.

### Chickens as Indicators of Virus Activity

Reeves: As examples, we had already been using tests on blood samples from chickens in farmers' flocks to indicate how much virus activity there was. If you went into a farm flock, you could bleed chickens that were one year or less of age; they had just been through one summer. You could test the bloods for antibodies to encephalitis viruses and utilize this information as an indicator of virus activity during the past summer. They were quite sensitive sentinels. We didn't have to put flocks out, because a lot of farmers and people in urban areas still had flocks in their backyards. So we'd use their chickens as our indicators and convince them that it was important to let us have blood samples.



Hughes: Is that the same thing as a sentinel chicken?

Reeves: No, we really were not using chickens as sentinels as we do now. We were only bleeding chickens at the end of the summer. In other words, we wanted to know where virus was active, and you couldn't collect mosquitoes everywhere. We found that we still could do a sampling of chickens from many places in the county at the end of the season--say, in November. If you found that 90 percent of the chickens were immune at one site after one season's exposure, and at another site only 10 percent or none were positive, there was a real difference. Generally the urban flocks had much lower infection rates than the rural flocks did.

When we really were doing intensive control and wanted to evaluate it, we started using sentinel chickens. We built our own sentinel chicken coops and put them in places where we were evaluating control. So in a ten-square-mile area we could have ten or fifteen flocks of chickens if we wanted to. We also learned that if we bled them periodically through the season, we could find out when they developed antibodies, which meant they'd been infected at some defined period in the summer.

We also used chickens as sentinels in a different fashion about this time. We had always assumed that any infected mosquito was a bad mosquito. We developed a new trap that we called a bait can. We found that bakeries had many empty fifty-pound lard cans that were the source of the lard they used to produce delicious goods and give everybody coronary artery disease. When they'd used up the lard, they threw the cans away. Nobody had found any use for empty lard cans except us, and the bakeries would give them to us. We could cut out the top and the bottom and put an inverted screen cone in. Then if we put a chicken or dry ice as bait inside the can, it functioned just like a fish trap. The mosquitoes would go into the can for that bait, and they couldn't find their way back out.

We started putting three-week-old chicks in the cans at night to learn how many mosquitoes bit a chicken in one night. Then we could also test those mosquitoes to see if they were infected with virus. Then if we tested the blood of the chicken for virus or antibodies, we could find out if the mosquitoes that fed on the chicken that night had also transmitted the virus. We found that only about one in four of the infected mosquitoes was able to transmit the virus. That meant that every mosquito that was infected wasn't a bad mosquito. That's also one of the ways that we first got life table data and found that most mosquitoes that got infected died before they could transmit the virus.

Virus Transmission by Mosquitoes

Hughes: You mean that any given mosquito could never transmit a virus, or that at the moment you were testing it wasn't in a position to transmit?

Reeves: At the moment that it bit the chicken, many were infected but couldn't transmit the infection.

Hughes: But earlier or later it could have transmitted?

Reeves: Our knowledge at that time said earlier it couldn't have transmitted. Now we know that with western virus a mosquito can cure itself of infection. We'll get to that later when we talk about vector competence. So a mosquito could have transmitted and later cured itself, but that we didn't know at the time. What it meant generally was that a mosquito that fed on virus then had to go through an incubation period before the virus would get from the gut into the salivary glands, and we knew that. We also knew that it took a period of eight to ten days at summer temperatures.

Hughes: Had your group worked that out?

Reeves: Yes.

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Reeves: We hadn't worked out details of the life table of the mosquito, which showed later that, say, 30 percent per day died. So if 30 percent of the mosquitoes in a population die each day, then most of the ones that get infected on the second or third day of their lives were just not going to be there long enough to transmit by their bite a week or so later.

Not only that, but we worked out later that by the second blood meal, they might not have gotten to the point where they could transmit the virus. They may have to have three blood meals to transmit the virus. Now they're old mosquitoes, and there are not very many old mosquitoes in a population. So the more we learn about this, the more it seems unlikely the virus can even exist.

Hughes: Yes, but we know it does.

Reeves: But it does, yes. That means you're up to here in mosquitoes.

Hughes: Yes. And that's why encephalitis epidemics don't happen very often.

Reeves: Yes, that's true, because you have to have certain levels of mosquito populations. Everything has to be right.

### Writing the 1962 Monograph with Dr. Hammon

Reeves: When we began to bring this information all together, we published our first monograph, which was for the period from 1943 to 1952.

Hughes: That's the 1962 monograph?<sup>1</sup>

Reeves: The 1962 monograph. But it only went up to 1952. People asked, "Why didn't you put it out in 1953?" Well, Hammon had gone to Pittsburgh in 1949, so our years of study with him went up until 1949. Then he left, and we got busy with epidemics and other activities; so, frankly, we were pretty busy and didn't get around to it. It really wasn't until the late 1950s that Bill Hammon and I met someplace and decided we ought to put the first ten years of our research together.

We started working on that project in about '58 or '59. Another delay was that we had another small epidemic of western encephalitis in 1958, which we managed to abort through good mosquito control. Meanwhile, Hammon was working on some aspects of the monograph in Pittsburgh, and I was working on other aspects here. We weren't making very fast progress working apart, so in 1961 we decided that we were going to have to do something about this and get it done. He said, "Okay, I'll come out to California. I want to get away from all of the attention I'm getting from newspapers here in Pittsburgh." This was the time of the big problem with the Cutter incident and polio vaccines.<sup>2</sup>

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<sup>1</sup>W. C. Reeves, W. McD. Hammon. Epidemiology of the Arthropod-borne Viral Encephalitides in Kern County, California, 1943-1952. University of California Publications in Public Health. University of California Press, Berkeley, 1962.

<sup>2</sup>See the oral histories of Harald Johnson and Edwin Lennette for discussion of problems with polio vaccines.



His problem was that he had been in the middle of polio work because he'd done the gamma globulin study in Texas, which I talked about earlier. So he was one of the authorities on vaccines and immunity for polio. It was a peculiar situation, because Hammon was working out of Pittsburgh, and sitting in the next building was Jonas Salk, who was developing the Salk vaccine. The two of them weren't the closest of friends, because they'd had some conflicts in the earlier work on polio--I mean difference of opinions and so on. A lot of prima donnas in this business. Albert Sabin really wasn't into polio vaccine work with both feet yet; that came later.

Anyway, it wound up that big lawsuits were being processed against the Cutter Laboratories, and Hammon had become good friends with the Cutter brothers when he was here and had done a lot of consulting work for them. So they had gotten him as their primary defense witness. Then, to the amazement of a lot of people, Jonas Salk became a primary witness for the plaintiff. That's a whole other chapter that I prefer not to go into any further than that.

Hughes: Why does it surprise you that Salk would be on the other side?

Reeves: He's the one who wrote the basic recipe that Cutter used and NIH endorsed to make the vaccine.

Hughes: Yes, but didn't he maintain that they didn't follow it?

Reeves: He maintained that, but there was no proof that they didn't follow it. However, I said I didn't want to discuss this further. The important thing is that Hammon wanted to get away from the newspaper reporters; so he came out here, and we were going to work on the monograph. He'd been here about two days, and the reporters caught up with him. We'd sit in my office trying to work on the monograph, and the phone would ring or somebody would bang on the door, and a bunch of people with cameras would show up.

After we did that a couple of days, we decided we had to get away from it. He was staying at my house, and that night we decided to skip the country. I came into the office and packed up all the reprints and data we were going to need, a typewriter and plenty of paper, and a calculator. We packed all that material and our fishing rods, because we both liked to fish, and we

disappeared. My wife didn't know where we were. His wife didn't know where he was. Nobody at the lab knew.

We went to Lake Almanor and got a very nice motel room. It actually was a suite, as we had two bedrooms, a kitchen, and a living room in a little motel. It probably cost us an exorbitant thirty dollars a day. We spent two weeks up there.

Hughes: And you wrote the monograph?

Reeves: We wrote the monograph. It was very efficient. We'd get up in the morning, and I'd cook breakfast and we'd eat. We'd go to work about six in the morning, and we'd work until about ten or eleven. We'd be somewhat tired, so we'd go out and go fishing for trout for a couple of hours. I taught him how to catch trout on a dry fly. We'd come back and take a siesta, and then we'd work until about six. So we were putting in eight- or nine-hour days. There were no movie houses within miles of where we were. We went fishing again in the evening.

The only problem we had was that the first night we got up there we decided we didn't want to cook yet, so we went to a little restaurant that was down the street. They had an interesting menu with meatloaf and other dishes on it, so we ate identical dinners. I woke up at two o'clock that morning, and man, I was sick. I had the most beautiful case of food poisoning that you ever saw in your life. I had hardly gotten out of the bathroom from my first spate, and I heard his feet hit the floor. [laughter] Fortunately, we were synchronized. We took turns.

The next morning when we woke up, we were as limp as a couple of sick cats. I'd been to the bathroom last and was feeling better than he was, so I took off to go down to this little neighborhood grocery store to get something that would stop all this. Pepto-Bismol was all they had on their shelf. I bought all the Pepto-Bismol they had. We filled up with Pepto-Bismol, and that stopped it all.

We were feeling good enough to eat again that night, so we went back to the same restaurant, and they had the same things on the menu. To show you how crazy we were at the time, we divided the menu. I'd eat the meatloaf, say, and he'd eat the pie. We each took a high-risk food. But we didn't get sick again because we probably couldn't. We were worn out.

The reason we did this, as I related earlier, was that one of Hammon's major studies at Harvard was on staphylococcal toxins in Boston cream pies, a very appropriate topic for that area, so he was one of the world's authorities on food poisoning. As a matter

of fact, he'd developed the kitten test for staphylococcal toxin, where they would take the food, make a filtrate, inoculate it into a kitten, and the kitten would get food poisoning. Anyway, that's a whole other chapter. But we got the monograph done.

We were able to put together ten years of solid data on all aspects of the epidemiology of these infections--the age and sex distributions of the human disease, immunity rates from inapparent infections versus apparent infections, the correlations with temperature that are required for a virus to incubate in the mosquitoes, correlations of mosquito populations with water and high temperature availability, etc., etc. We were able to show that there was a whole group of factors that correlated with the occurrence of epidemics or increased virus activity. In years when there was a low risk of virus transmission, there was very little water, comparatively low temperatures, low mosquito populations, etc. By now we had data from Kern County on mosquito density from light traps. We were able to use data on antibodies in sentinel chickens. We had knowledge of the many factors that influence disease, inapparent infection, apparent infection, and climatic factors. We really had the building blocks that allowed the development of a comprehensive surveillance system.

#### More on the Encephalitis Surveillance Program

Hughes: How did you recruit participation?

Reeves: By talk. There wasn't much else to do to get participation. I mean, the physicians had hospitals sometimes full of cases, and they wanted to get diagnoses done, so it was no trouble getting them to take diagnostic specimens. Anyone who developed encephalitis in Kern County severe enough to require hospitalization had to go to Kern General Hospital. It was the only hospital in Kern County that had an isolation ward. In those days we didn't know enough about the infectivity of a case of encephalitis, or we couldn't separate encephalitis from polio, and polio cases all had to be isolated; so the encephalitis cases just got put in the same wards.

Now, that wasn't too good an idea, because on occasion there was pretty good reason to think that a few kids got infected with polio in the hospital as a result of being put on the same ward with polio cases. However, Kern General had a good nursing and medical staff, so the risk of infection remained low.



During the summertime we would get a couple of medical students down to Kern County to work with us in the field, and every day we would have them go to the hospital to coordinate collecting the necessary blood samples for encephalitis and the stool samples for polio. So we threw some extra help in, which the hospitals always appreciated. The students also abstracted the clinical records and so on.

Hughes: Was the surveillance system just for Kern County?

Reeves: At that time, yes--say from 1952 into the 1960s. We were expanding it constantly. There was a big step-up in the State Health Department's efforts to follow up on the clinical cases. But we began to realize that a surveillance system could not be based only on detecting human cases; you couldn't use that information to predict an epidemic, because by the time you had your first human cases, things were out of hand--and you didn't want human cases. The detection of human cases is an effective surveillance system for diseases that are spread from person to person, such as measles or polio, but in encephalitis a human case is nothing but an accident when a person happens to have been in a situation where virus is present in birds and in mosquitoes.

Hughes: So what are you looking at, then?

Reeves: You have to get ahead of the time when the human infection will take place. By this time we were learning that if mosquito populations were low, you could still have virus activity. You could measure that by isolating virus from mosquitoes, or you could detect it by surveillance of infection in sentinel chickens. The birds would get immunity from having an inapparent infection, but they didn't get sick.

So basically we were learning that for effective transmission, virus activity in the mosquito vectors and avian hosts had to be high, and this usually would precede the occurrence of human and/or horse cases.

At this time we still were working at the county level, and we had the advantage that we knew the viruses had been active in Kern County since the late 1930s when Miss Howitt first detected them there. Both viruses had been active every year. So we had a natural laboratory in the field in which we could study these events and the sequence of their occurrence.

Hughes: Was it your group that was mainly responsible for watching for these factors that preceded human infection?

Reeves: Yes. We were the primary group that was doing this, and this was really the primary basis for our research grant with NIH.

Collaborating with the Centers for Disease Control

Reeves: The other groups that were working on encephalitis at this time were principally at the Centers for Disease Control, but most of their effort was put into chasing epidemics. The CDC was considered to be a state service type of an organization. So there would be an epidemic, and they'd be called to Florida to chase a St. Louis encephalitis epidemic, or to some place in Texas if there was an epidemic there. They were always getting into the epidemics after they were well started. They were trying to diagnose the type and size of the epidemic and to show which mosquito species was involved as a vector. We were not chasing epidemics; we were chasing what you might call the endemic aspect of this disease, namely its natural cycle. So we were really the only group that was spending twelve months out of the year in the field gathering data on any aspect that we could of the basic infection cycle.

Hughes: Was this looked upon as a two-pronged approach, in the sense that you were looking at the endemic aspect and CDC was chasing epidemics? Or did it just work out that way?

Reeves: CDC was chasing epidemics because that was a major part of their job. One of the problems CDC had, and they complained about it sometimes, was that they would set up field programs in endemic areas doing many of the same things that we were doing. For instance, they had work that was going on in Texas and in Colorado. They would just get a good field program going in the summertime, and there would be a major epidemic someplace else. They would then be pulled off of their research program to study the epidemic, and they couldn't have a continuity of their ongoing endemic studies.

Hughes: You were giving the continuity, weren't you?

Reeves: We didn't have an obligation at this stage to go someplace in some other state to study epidemics; that was CDC's job. So we weren't competing. They were doing many of the same things that we were doing on how mosquitoes became infected and how effectively. Dr. Roy Chamberlain and Dr. Danny Sudia were doing a lot of laboratory studies with vectors at the same time we were. You also have to realize that they were covering eastern equine encephalitis and that we weren't concerned with that virus at all.



So they were following eastern, St. Louis, and western encephalitis epidemics. They were spending a lot of time chasing epidemics and just couldn't keep their noses to the grindstone like we could.

Hughes: Was their methodology essentially yours?

Reeves: Essentially methodologies were developing in parallel. We were exchanging information freely. Sometimes they or other workers would come up with a new methodology. For instance, when we started our studies, the neutralization and complement fixation tests were the two tests used for diagnosis. And then in the sixties Albert Sabin came up with the hemagglutination inhibition test, and that became used in all of our laboratories. The Rockefeller Foundation started pushing the use of suckling mice instead of 21-day-old mice as being a more sensitive way to isolate virus, and we all followed that advantage. It was a community of workers that was exchanging methodologies, even exchanging staff.

Hughes: Were there ever problems with one side or the other feeling that the other side was overstepping?

Reeves: Not that I was conscious of. It's amazing that we didn't have more trouble. There may have been jealousies. I never had any feeling of that type or that we were stepping on each other, or even that we were competing with each other. There could have been people who thought they were competing.

#### Prediction of an Encephalitis Epidemic in 1969

Reeves: In 1969 we had predicted that an epidemic of encephalitis might happen in California. The reasons were that again we had a period of very high water availability, and *Culex tarsalis* were coming out of our ears anyplace that we looked. I was asked by the mosquito control agencies if I wanted to get on my old, worn-out stump and yell, "Epidemic!" They seemed to think I had a loud voice when I did so; so I did, and the press picked it up.

We had many people in the state very upset and alert. The state legislature committed an extra million dollars for emergency mosquito control, and even Governor Reagan signed it, which was amazing. The idea was that the supplementary money would be used in areas where there was no organized mosquito control, because the organized mosquito control districts had pretty good programs and reserve money they could use for extra control.



At that time we still were working primarily in Bakersfield. The idea was that the surveillance of what was going on in the state ought to be rapidly expanded--not just to be in Kern County, but to be almost a statewide program. So the CDC sent Dr. Danny Sudia out from Atlanta, Georgia, to be the honcho, the head guy, to organize mosquito collecting all over the state, which he did. He stepped on a lot of toes while he was here. Not on my toes--it didn't bother me any--but on the toes of some of the mosquito abatement districts. He'd make an appointment to spend time in their districts and wouldn't show up on time because he was too busy someplace else and didn't have enough help.

They collected thousands of mosquitoes that summer, just thousands of mosquitoes, and we did, too. For practical purposes, there was no virus activity. It was very obvious by the first of August that nothing was going to happen.

Hughes: Why?

Reeves: That was the question, "Why? Why isn't anything happening? The mosquitoes are here, there are more mosquitoes than we've seen since 1952 or 1958." Those were the years when the viruses had gotten out of hand completely. Everything looked like it should be "go" in 1969, and nothing happened. Sudia went home in considerable disgust, because he'd done all this work to collect 100,000 or more mosquitoes, and no virus of any significance was gotten out of them.

I sat around and scratched my head--I say "head" advisedly [laughter]--and worried about what had happened. Everybody was yelling at me, "Why didn't it happen? Why didn't it happen?" There was really only one thing that we came up with at that time that would explain why not too much happened, even though we had flooding that covered the whole Central Valley. I have pictures of thousands of acres of flooded farmland that never got farmed that year, and there were mosquitoes everywhere. The mosquito control workers really did intensive control, but mostly in urban areas. They just couldn't cover the other areas. When I went out into the rural areas of Kern County, it was very impressive that almost every female mosquito I collected out of shelters contained eggs, which meant they were slightly older mosquitoes and hadn't just hatched. They almost all had eggs in them. When we found mosquitoes in the field that had eggs in them, we had always thought that it meant they had taken a blood meal.

### Autogeny

Reeves: But meanwhile, we'd also discovered in one of our laboratory colonies that there was a condition called autogeny, which is a genetic trait. If *Culex tarsalis* is raised in a very rich environment--that is, water with a lot of food in it--the larvae will store up enough fats and protein in their bodies so the females can develop a first batch of eggs without taking a blood meal. So they don't have to have blood to develop the first batch of eggs. We call that autogeny. The field population that year had extremely high autogeny rates.

You ask, "What does that have to do with the time of day?" Well, if the female, when it hatches, is ready to and develops eggs, then it's anywhere from four to five days old before it will take its first blood meal. So the first blood meal is delayed, and they go ahead and lay their first eggs. Then they can take their first blood, and that has to have virus in it for them to become infected. After they oviposit they take their second blood meal. However, they're not ready to transmit infection yet, as the virus incubation is incomplete. So it isn't until the third blood feeding to prepare for the fourth egg batch that they can transmit virus--that is, if they picked it up in their first blood meal. So it's back again to the whole business of the importance of knowing how long mosquito populations live. Relatively few survive more than a few days. The western and St. Louis viruses we were working with are not transmitted with any frequency to the eggs, so transovarian transmission will not get around this autogeny problem. And autogeny certainly suppresses virus transmission, as it delays the first blood feeding, which can be a source of virus infection.

We also couldn't understand why in some years there was a lot of virus activity in the southern San Joaquin Valley, but we didn't have a parallel high level of virus activity in the Sacramento Valley. Yet there were much higher populations of *Culex tarsalis* in the Sacramento Valley than in the San Joaquin Valley. There was a lot more water up there with the rice fields and a lot more river flooding. It's an area of water surplus. As they say, "We have a flood every year in the Sacramento Valley." Sometimes it's irrigation flooding, sometimes it's natural flooding where everything may go underwater. Well, autogeny helped to explain it to some extent but not completely.

### Interactions of Birds and Mosquitoes

Reeves: We also made a very interesting discovery in Kern County, which we could apply later in the Sacramento Valley. When we used the bait can traps to attract mosquitoes to feed on chickens and to measure virus infection and transmission rates, we found that when a very large number of mosquitoes went into these traps, comparatively few would succeed in feeding. For instance, if a hundred mosquitoes went into a trap, ninety might feed. If a thousand mosquitoes went in there, only 10 percent would feed. There would be a hundred mosquitoes fed in both cases, but the other nine hundred in example two didn't get blood. So again this took some explaining. I mean, why was it that the more mosquitoes you had, the fewer got a blood meal?

Again, to make a long story short, we found out that if we wrapped the three-week-old chicks in silk or nylon stockings so they couldn't move, almost all the mosquitoes fed. What was happening was that the unbound chickens were fighting the mosquitoes off. They wouldn't go to sleep at night, and they'd fluff up their feathers and move around. This is what they also do in nature.

We found we could use the precipitin test to confirm this phenomenon. When *Culex tarsalis* was at a low population, the field population all fed on birds, for practical purposes. If the population went up in numbers, they started feeding more and more on mammals--cows, horses, dogs, people--whatever was around. If there are two species of mosquitoes in an area that feed on birds, say *Culex tarsalis* and *Culex quinquefasciatus*, that can increase the aggravation and reaction of birds.

Now, what happens is that you get to a level of mosquitoes where the sheer numbers of the mosquitoes decrease the proportion of feedings on birds, which are the important source of virus. The mosquitoes are diverted to feeding on mammals that usually don't have virus in their bloodstream, and it interrupts virus transmission. So you have a situation where there are too many mosquitoes in an area, and it isn't very good for virus transmission, contrary to everything you have believed. Everything's wrong, but that's the way it works. This finding upset people who were sure that the more mosquitoes there were, the more virus transmission and cases of disease would occur.



That was so for malaria and epidemic yellow fever. Our first paper on this new finding got some attention.<sup>1</sup>

I got off on a tangent there.

### Avoiding an Encephalitis Epidemic in 1969

Hughes: Well, how did you explain why the 1969 epidemic wasn't an epidemic?

Reeves: Many of our indicators told us in '69 of the likelihood that there might be an epidemic. This also taught us another lesson, that you can't predict for sure when an epidemic is going to happen. You can predict when an epidemic is not likely to happen. The difference here is that if most factors are negative and not favorable for a virus to be transmitted effectively, you can say, "Relax, enjoy life." But if things go to the other extreme, you may have an epidemic if the balance is just right, or you may get to the point where you have a surplus of mosquitoes and very little is going to happen.

Hughes: You can't examine that situation closely enough, early enough, to be able to predict?

Reeves: Only in the sense that if you begin to build up a big mosquito population early in the year and you don't find virus activity. If you can't find virus in the mosquitoes or you can't find virus in your sentinel birds, there's nothing happening, so you can tell that early. Actually, in 1969 we stopped the emergency control program in August of that year because that was the case.

Hughes: You couldn't find virus activity?

Reeves: Yes, and we gave some money back. That was the famous time when Governor Reagan said, "Why did you ask for it if you didn't need it?" I never did learn how to answer that man. [laughter]

Hughes: Neither did a lot of people.

Reeves: You said that, I didn't.

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<sup>1</sup>W. C. Reeves. "Mosquito vector and vertebrate host interaction: the key to maintenance of certain arboviruses." In The Ecology and Physiology of Parasites, A. M. Fallis, ed., University of Toronto Press, Toronto 1971, pp. 227-231.

People said, "We thought you were smarter than that." It was the second and maybe the last time I gave money back. [laughs] After that, people said, "We'll never give money back. We'll find some use for it."

Hughes: Dr. Reeves, could you recap your research surveillance system?

Reeves: To do so I will have to go back and summarize some work before 1969. In 1958 it seemed we had an impending epidemic in Kern County. At least our scanty surveillance data told us it was going to happen. We had very high mosquito infection rates in May.

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Reeves: Almost every pool of fifty mosquitoes was positive in the urban area of Bakersfield. We didn't have any cases yet, but again, it was a year of flooding, there were a lot of mosquitoes in the urban area, and virus was active. We had a meeting with Art Geib of the Kern Mosquito Abatement District and health department staffs, and we agreed, "We really have to do something." So it was decided to concentrate the control program to try to protect the urban population of Bakersfield. There weren't resources to establish intensive control throughout the county, and the urban population seemed to be at greatest risk.

One of our major problems was that there was a flood by that time all along the Kern River. The release of water from Lake Isabella by a flood control dam built on the Kern River was controlled by the U.S. Army Corps of Engineers. They were turning all the excess water they had in Lake Isabella down into the Kern River and flooding the area, even flooding part of the city of Bakersfield. The water went over the levees. We actually tried by phone to talk them into cutting down on the water flow and releasing it later in the year. They said, "No, we can't do that because the agricultural water users would all have to agree to this, and we have to be sure we can have enough storage space behind the dam next year if there is another big snowpack."

The Kern Land and Cattle Company, the largest water user, wanted to stop the release of excess water because a lot of their agricultural land was being flooded. So the company loaded Art Geib and me and their staff into their private airplane and took us up to Sacramento. The water users, we the researchers, and the Army Corps of Engineers met to discuss turning down the water. The army didn't want to do it, and I finally said, "Look, if you don't shut that water down, save it and turn it loose later this winter when it can be taken into the groundwater table and so on,

I'm going to have to tell the newspapers that you're responsible for the epidemic when it develops." That did it. They said, "You win." So they turned the water down, and the mosquito abatement district did intensive control, and we really aborted that epidemic.

### More on Surveillance

Reeves: In the early sixties we did some very intensive additional field studies on experimental control in extensive areas of the west side of Kern County to see what levels of mosquito populations we had to get down to in order to stop virus transmission. In these studies we really established the components for a surveillance program, including use of sentinel chickens and establishing measurement of the levels of mosquito populations, virus infection, water availability, temperatures, and financial resources that the mosquito abatement district had to have for control. All of these things were important information in a surveillance system.

We evaluated those factors and demonstrated that indeed you could knock a mosquito population down to a low level where virus activity would actually stop. That level was very low in that it was less than five mosquitoes per trap night, which is a very small population. This information was given out to various control agencies in the sixties, and it indicated you could organize a surveillance system and use it if you wanted to.

By 1970, the mosquito abatement districts were ready to go on an almost statewide surveillance system, and these organizations had been conditioned to the fact that it was in their interest to get this information. So a grand collaboration system was initiated in which the mosquito abatement districts would collect mosquitoes, identify and make up pools, freeze them, and send them to the state laboratory to be tested for virus. Sentinel chicken flocks were established in 1979 in many areas, and blood samples were sent to the state lab monthly to be tested for antibodies. The districts also would provide data to the statewide program on the levels of mosquito populations occurring in light traps. So it wasn't just a matter that one county was doing surveillance; many districts in the state, particularly in the Central Valley, were doing it. The state was set up to do the human and horse diagnoses, chicken sera, and mosquito tests. So really, all the pieces began to come together. Actually, we weren't involved very much at that stage in doing the day-by-day things like collecting the mosquitoes, making up pools, or the virus testing. This was



all being done as a state program by collaboration with local agencies. At times we did add persons to the staff of the state laboratory during heavy work periods or did supplementary testing in our laboratory or made collections for testing.

What we did primarily was to continue our basic research on life tables of mosquitoes, new approaches to mosquito control, and studies of how the viruses get through the winter. We were still trying to pick up as research problems the questions that we didn't know the answers to.

Hughes: So that really was the beginning of the statewide surveillance system. Did it follow the various criteria that you had proposed?

Reeves: Yes. In the sixties we had meetings in the state that formalized the surveillance system. By 1980, they really had a statewide surveillance system going in most areas. They just kept gradually expanding the surveillance area. In 1983 the first document was prepared that described details of the surveillance system.<sup>1</sup> A revised edition was issued in 1989 that included recommendations for urban areas.

Hughes: Was there any penalty for noncompliance?

Reeves: No. It was all voluntary. You cannot have a law that people have to do this sort of thing. You may have a law that a physician who diagnoses a case of encephalitis has to report it, but no one has ever been prosecuted for not doing it, because they always have an out. They say, "I wasn't sure yet. If I'd been sure, I would have reported it."

Hughes: How was compliance?

Reeves: When we were having a large number of human cases, it was good. I would say that the weakest link now in the surveillance system is the diagnosis and reporting of cases by physicians and veterinarians. The reason is that encephalitis is not as common a disease as it was before. We've gone for a number of years now with a very small number of cases, and when we have detected flare-ups, it's usually been in retrospect.

In 1984 we had twenty-some cases of St. Louis encephalitis in the Los Angeles metropolitan area for the first time in the history of the state. That summer we knew there was St. Louis

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<sup>1</sup>California's Mosquito-borne Encephalitis Virus Surveillance and Control Program. Published by State of California Dept. of Health Services, Vector Control and Surveillance Branch. 1983.

virus activity, because sentinel chickens had converted and some mosquito pools were positive. We didn't know anything about human cases. It turned out the laboratory diagnoses were all being done by a private laboratory. They didn't believe that it was their job to report the cases. They reported the diagnoses back to the physician, and we learned about it in retrospect after the outbreak was almost over.

Hughes: Encephalitis was no longer being confused with polio, was it?

Reeves: It could have been clinically, but it's not confused with polio today, primarily because of the polio vaccination program. Generally they don't even suspect polio now. They're more likely to suspect encephalitis. There is a high priority for diagnosis of every polio case, as it represents a breakdown in the effort to eradicate the disease.

Hughes: What could encephalitis be confused with?

Reeves: It usually gets thrown into what I call a diagnostic wastepaper basket, aseptic meningitis. As I mentioned earlier, aseptic meningitis means inflammation of the meninges of the brain, a clinical diagnosis, and that they have tried to culture bacteria or fungi from the patient and they can't, so it's aseptic.

Hughes: But they haven't taken the next step, have they?

Reeves: They haven't necessarily taken the next step. So it could be viral, it could be chemical, it could be protozoal, it could even be malaria affecting the brain; it could be anything that will cause inflammation of the brain.

Hughes: Why don't they carry the diagnosis further?

Reeves: The problem is, if encephalitis is diagnosed specifically as western or St. Louis, it doesn't change its treatment. We have no vaccine to prevent human cases, so we couldn't have prevented it by vaccination. Secondly, there is no antiviral substance that they can give to the patient that will help him, so the treatment is purely symptomatic--to prevent pneumonia with antibiotics, give good hospital care, try to stop convulsions, and things of that type. So really they're treating the cases by the seat of their pants by whatever the symptoms are and alleviating those as much as they can, trying to bring the fevers and headaches down. There's nothing specific they can do, so from a clinical viewpoint, what's the difference?

Hughes: Is that upsetting to you?



Reeves: It's not my privilege to be upset about it, because they would say my interests are self-serving in the sense that I want all the information for research purposes. I get the same reaction from some mosquito abatement districts: that I'm interested in getting the record on mosquitoes, viruses, and so on, for research. I can't shake that in some people's minds. In fact, I like to think I'm a little bit more of a human being than that, but I also think a specific diagnosis is important if we are to have a good surveillance program and evaluate the success of the vector control program.

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Hughes: I understand that there are five categories of information that are monitored concerning western and St. Louis viruses.

Reeves: Yes. Actually, we put these into a frequency distribution [time line] based on the order in which they come into our consideration each year.

The first main category is what sort of resources we have for vector control in an area. That may seem to be sort of an indirect factor, but if you don't have resources for vector control, there's not much use in having a surveillance system. If you're going to use a surveillance system to anticipate an epidemic, you've got to have an organization that can respond. We are very fortunate in that regard in that we have rather detailed data available to us. Mosquito abatement districts are financed on an annual basis, on a tax basis. They can tell you in advance how much money they will have, how much money they have in reserve, what staff, equipment, and facilities they have, and so on. So that information really is a part of the surveillance system. We know geographical areas that aren't covered, so if you have an epidemic pending, some other agency like the State Department of Health Services is going to have to take over and control mosquitoes in unincorporated areas.

The second category is water availability. I've stressed the importance of snowfall and rainfall as sources of water, but also we are concerned with the developments that have taken place in California with regards to the movement of water from northern to southern areas for agricultural irrigation. We have detailed data on where water is going to be available. Right now I'm not too worried about how much excess water we're going to have available from flooding or from any other source in California, because we're going into a fifth year of drought.

The third category is temperature. The U.S. Weather Bureau collects temperature data and gives it to us for free. You can



get data on a daily basis from weather stations all over California. You can get average temperatures accumulated over the years. So we use this information as a database, which is in computers and readily available.

The fourth area is vector population levels, and here we concentrate on *Culex tarsalis* as the primary vector of importance, but we don't ignore the other *Culex* or *Aedes* that can be secondary vectors. We get data from light traps all over the state, which does not give you a census of mosquitoes but gives you the ups and downs in populations. So we have population levels based on a trap index per night. We say, "These are very low levels, so the virus may disappear; or these are population levels where virus can be transmitted at a low level but not levels where virus can be transmitted very efficiently. Finally, here's a year with an upper level of mosquito populations where the virus can start being dampened by the mosquitoes changing their feeding habits."

The fifth area that we worry about is the level of the virus activity. The first four areas all come first in the sense that you can get an idea of what they are before you detect any virus activity in the mosquito or transmission to a sentinel chicken. So next we use vector sampling for virus isolations: if we isolate virus, we know it is here and of what type, in which mosquito species, and exactly where it is. It can go from very low levels of activity in the vectors to very high levels. We have statistics that we can use to refine the data on vector infection rates.<sup>1</sup>

In California we use chickens as our primary sentinel hosts, although in some areas people use wild birds. We did the first work on using wild birds as indicators of virus activity. The difficulty with wild birds is that you don't know how old they are, because sometimes it's hard to tell an old bird from a young bird, and you don't know anything about where they have been unless they have been marked with a leg band. If anything happens in a sentinel chicken, you know it happened right there where the bird was placed.

Hughes: Why would people bother with wild birds?

Reeves: They don't want to be bothered with chickens. They think wild birds are more sensitive sentinels. We don't happen to think they are, and we don't think chickens are that much bother. I think

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<sup>1</sup>C. L. Chiang, W. C. Reeves. Statistical estimation of virus infection rates in mosquito vector populations. Am. J. Hyg. 75:377-391, 1962.

it's a lot easier to have a sentinel flock of chickens sitting out there being fed and watered and which will be right there if you want to bleed them than it is to try to catch wild birds, which maybe you can catch and maybe you can't. You don't have to have permits to net chickens. You have to have permits from State Fish & Game and federal agencies to trap wild birds (and other animals). If you want to use wild birds as sentinels, you should catch them, put a leg band on them so you can identify them, and catch them again. And the odds of catching ones you've banded and turned loose get slimmer, slimmer, and slimmer as time goes by. I think that wild birds are a shaky method, but other people don't necessarily agree with me.

And then, finally, you have human cases as sentinels of the level of virus activity. When cases occur, that is what you don't want to happen; that is what you're trying to prevent, so that's sort of the last information you want. Also, as we discussed earlier, I think physicians are not that concerned about the diagnosis of encephalitis. Or if they're not using central laboratories, then you're not going to know fast enough that a case has occurred. It's not practical to wait for human cases to occur before you start mosquito control.

Disease in horses used to be a good sentinel for the presence of western encephalitis but has become almost worthless to us as a sentinel because of vaccinations, decrease in the horse populations, and the fact that horses now are almost a suburban animal; they're not a rural animal. They're pets in suburban areas and get a lot of veterinary attention, get a lot of vaccinations.

#### The 1984 Epidemic in Los Angeles##

Reeves: Actually, during the epidemic of St. Louis in Los Angeles in 1984, some people said the surveillance failed because an alarm system didn't go off, warning that there was going to be an epidemic of twenty-four cases of encephalitis in that area. The reason there wasn't an alarm is that historically we'd never had a series of cases in the metropolitan area of Los Angeles. There had been a few cases in Riverside, San Bernardino, and Imperial counties. Los Angeles was outside of the endemic area. It wasn't in the Central Valley, and it wasn't particularly *Culex tarsalis* country. There were only two sentinel chicken flocks in the whole metropolitan area, one in Orange County and one in Los Angeles County, and practically no pools of mosquitoes were being sent in.



Well, the chickens started converting. They did their best to yell, "Epidemic, epidemic." And some of the mosquito pools were positive. Then the critics said, "Well, the surveillance system wasn't good enough, because when we finally found out, some of the human cases had occurred before the chickens converted." My response was, "Well, what the hell do you expect a surveillance system to do for you if you have only two chicken flocks and a few mosquito collections turned in to the lab, and that is the total surveillance for a huge metropolitan area? How do you expect to know more than you did?"

Since that '84 experience, a larger number of sentinel chicken flocks were established in that metropolitan area, and a lot of mosquito pools have been tested. The surveillance system has detected virus every year, and there have been very few human cases in the urban area--ten in a seven-year period. The infection rate is low in the mosquitoes and chickens and not high enough to predict there would be an epidemic, but it has worked.

Hughes: Are both viruses detected?

Reeves: St. Louis only in the metropolitan area until 1989 and 1991, when western was detected in a chicken or occasional mosquito pool. In the Coachella-Imperial Valley, both western and St. Louis virus have been active. We can get to that later.

#### Research in Northern California, 1969-1974

Reeves: At about the same time the surveillance system was being beefed up in Kern County in the late 1960s, we got some real pressure from northern California. Some managers from the northern California mosquito abatement districts said, "Look, all this research is going on in Kern County, and we're getting tired of hearing about it. We want some research done up here."

They had some reason for saying that, because they didn't know for sure whether they had problems that were unique to their area. Actually, in 1969 and earlier there had been virus activity in the Sacramento Valley area. There hadn't been a lot of human cases, but there had been consistent virus activity in the vectors and sentinel birds. They'd just gone through the trauma of really being scared because of the threat of an epidemic in '69, when the state legislature had allocated a lot of supplemental money to be used for mosquito control during that supposed epidemic. One and a quarter million dollars had been allocated by the state for



emergency funding. The vector control districts in the state had spent over ten million dollars in vector control that year.

So they organized themselves as a group--Mel Oldham from Red Bluff, Bill Hazeltine from Butte County, and Gene Kauffman from Sutter-Yuba County. They said, "We want something to be done. We want you to do some research in northern California." I said, "We don't have enough money to do it. What are we going to use for people and other support?"

They continued to put the screws on, and to make a long story short, we knew that something had to be done to satisfy them. Actually, it was of research interest and in everyone's best interest to do it. So they managed to get the Chancellor's Office at Berkeley to add \$20,000 of supplemental money to our budget, over and above what we had from research grants and so on. I moved people from Bakersfield up to Chico in the Butte County District, and we rented ourselves a house to use as a laboratory. Bob Nelson, who had already been in the field program down in Bakersfield for seven or eight years, agreed in 1969 to go up there and be in charge of the new program. He took one of our best field people with him, Vince Martinez, and hired two vertebrate zoologists, Albert Beck and Mike Wright, and an entomologist, Richard Spadoni, to work with him.

So for five years (1969-1974) we did research in the Sacramento Valley. The principal objectives of our research were to confirm that what we'd learned in the Yakima Valley and in Kern County applied up there as well. And indeed we did. We found again that *Culex tarsalis* was the principal vector. A mosquito that was extremely common in the Sacramento Valley, *Anopheles freeborni*, which is one of the primary malaria vectors, wasn't involved at all in carrying western or St. Louis viruses. The very common *Aedes* mosquito, *Aedes melanimon*, which is a pasture mosquito, was somewhat involved, and we found the first evidence that this mosquito was infected in nature with western encephalitis virus.

In 1943 and '44, I told you that we had found a brand-new virus in *Aedes melanimon* in Kern County, which we called California encephalitis virus. But we had no evidence that this mosquito was involved as a vector of western or St. Louis viruses in Kern County. In the northern area we found we got both western and California viruses out of it, and with some frequency. As the studies continued, we found that there were a lot of jackrabbits in the pastures and wildlife refuges of northern California, and that *Culex tarsalis* carried western virus over from the bird cycle and into jackrabbits. *Culex tarsalis* liked to feed on jackrabbits as well as on birds. The jackrabbit, when it got infected, had

enough virus in its blood to infect a mosquito. *Aedes melanimon* preferred to feed on mammals, and the blacktail jackrabbit was one of the favorite blood sources for this mosquito. The western virus would go from the bird and *Culex tarsalis* cycle over into a jackrabbit cycle, which *Aedes melanimon* would pick up and transmit. *Aedes melanimon* also liked to feed on horses and people, so it potentially could transmit the virus over to them. So again, we extended our knowledge of the cycles by doing the Sacramento Valley studies.

Hughes: The same handful of people was doing this work?

Reeves: Yes, we had the people I listed up there year round and extra hands in the summer.

Hughes: Were you trotting up there every once in a while?

Reeves: Yes, I was trotting up there once in a while. The difficulty was that I couldn't trot up there very often, because during the 1967-1971 period I was dean of the School of Public Health, and problems on the Berkeley campus demanded my attention. As a matter of fact, that's one of the reasons that I quit being dean about that time, as I will explain later.

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Hughes: Dr. Reeves, how long did the Chico program last?

Reeves: We carried on the Chico program for five years. We were able to confirm the similarities between the data from Kern County and the northern area. We were able to show that sentinel chickens could be established in that area and would work effectively as a part of the state surveillance system, and that tests of mosquito pools for virus isolations could be productive. We also began to realize after five years up there that we'd answered most of the immediate questions that the mosquito abatement districts had. Also we were too short of resources to be able to put in the maximum effort on more basic problems in Kern County. So by mutual agreement we withdrew, but really not entirely, because I and other members of our staff, like Marilyn Milby, continued to go up there and do collaborative work in the field with the mosquito abatement districts, on publicity if they needed it, or on setting up biological field studies for their staffs. In many of those districts, particularly the Sutter-Yuba Mosquito Abatement District, we've done a lot of collaborative work. So we withdrew and went back to Kern County to put most of our efforts into that program as a continuing concentrated effort.

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Reeves: We had maintained the Bakersfield project and were carrying on research on the biology of *Culex tarsalis* as well as studies on birds, mammals, etc.

Hughes: When you say "biology," do you mean things like vector competence?

Reeves: Vector competence work was being done there and in Berkeley. Dr. Hardy was very much involved, and he was heading up the vector competence studies. He was doing experiments at Bakersfield, where we had established an experimental setup. We could do vector competence work there and also bring mosquitoes back to Berkeley to do vector competence work.

### Research on Mosquito Biology

Hughes: What falls into mosquito "biology"?

Reeves: The population numbers of mosquitoes, where they breed, how long they live (their life tables), what they feed on, the efficiency of control programs in knocking populations down, and how they overwinter. At this time we were starting to think of the possibility of getting involved in genetic control of *Culex tarsalis* populations. We were also doing extensive studies on the west side of Kern County, where desert was being transformed to farmland. A new canal was bringing water down from northern California. We found it took at least five years for birds, mosquitoes, and viruses to get established in this newly developed irrigated agricultural environment.

We still had an awful lot of things to learn. We had people at Bakersfield who were doing studies to see what impact the density of larval populations had on successful mosquito hatching; I mean, what numbers of the immature stages the water would support and still produce an adult mosquito population. For instance, if the levels of larvae were too high, would the mosquitoes that came out be puny and no longer effective mosquito vectors? Finally, we had begun to appreciate how important autogeny was as a factor in the biology of *Culex tarsalis*, autogeny being the ability of a mosquito to develop eggs without taking a blood meal.

There were too many things to be done, because we didn't understand the mosquitoes that well. It wasn't until we got into genetic control, which we'll talk about shortly, that we really got into aspects of mosquito biology that we had ignored for many years, such as how long males live, where and when male mosquitoes



mate with females. We always had ignored male mosquitoes; all the male mosquitoes went into the wastepaper basket, because we didn't expect them to be infected with virus. They don't carry diseases, they don't feed on bloods, they're not a pest; so who cares about male mosquitoes?

Hughes: Weren't there people anywhere studying mosquitoes just for the sake of knowing more about mosquitoes?

Reeves: Sure, there always are. I talked about John Edman, who was working in Florida and then moved on to Connecticut, where he became the head of the department of entomology at Connecticut University. He was and still is studying the interactions of birds and mosquitoes as far as the birds' defensive mechanisms are concerned.

Hughes: Is he interested in the pathogenic aspects?

Reeves: Not particularly.

Hughes: Were there other basic scientists who were studying mosquitoes as mosquitoes?

Reeves: There are people who study the classification of mosquitoes, their importance as food items for ducks and other animals, their genetics, and I could name some other areas. Rex Dadd in the entomology department was studying what size particles mosquito larvae could ingest.

In our field of disease relationships, Roy Chamberlain was doing very good work on vector competence at CDC in Atlanta, Georgia. The CDC group in Colorado was doing field studies on vector populations in Colorado and in Texas. They were doing experimental control studies on mosquito populations in Plainville, Texas. People like Carl Mitchell, Bruce D. Francy, and Richard O. Hayes were doing the studies in Colorado. Actually, Carl Mitchell left there and came to California for several years to be the coordinator in the University of California statewide program on mosquito research. He was here for several years and then went back to Colorado.

I'd hired Dick Hayes when he was a high school student, and he'd worked with us at Bakersfield in the summers. Then he went on for his doctoral degree at Cornell, and he came back and joined our staff in Kern County and became one of our field people there. Dick Hayes was the son of Fred Hayes, who had been the manager of the Kern Mosquito Abatement District when we started our work there in 1943.

### Exchanging Information

Hughes: Was there a lively exchange of information amongst this group, even pre-publication?

Reeves: Yes. There was a very tight network of information exchange. I don't think there was anything that came to publication that we didn't already know about. This was for several reasons that we haven't talked about. The American Committee on Arthropod-borne Viruses had set up a network of communication called the Arbovirus Newsletter, which was not only nationwide but international. Those newsletters came out frequently. Of course, we also published in the same journals, so sometimes if we didn't know about something, we would learn by refereeing the articles before they appeared, because the editors knew who was working on various aspects of arbovirology and mosquito biology. So Chamberlain would referee our vector competence papers, and we'd referee his vector competence papers. It was no secret what we were doing. Referees aren't supposed to be known, but they are.

Hughes: I know you were a member of the American Committee on Arthropod-borne Viruses. Did you find it a good source of new information?

Reeves: Yes. It provided a good exchange of information about what had been found. It was a good information exchange on methodology. For instance, when we developed the bait can to trap mosquitoes that were biting animals, we found out we could put dry ice (carbon dioxide) in the bait can instead of putting in the birds. Well, within the year that we first did this, people in CDC were using this technique in Florida and elsewhere.

After all, we were collaborating with CDC. Buck Bellamy, who had been the head of the field station at Bakersfield, was a CDC employee, so he wrote quarterly reports to CDC which were distributed to the virus laboratories of the CDC in Colorado, Kansas, and Georgia. And we got their quarterly reports in return. So CDC was forcing that exchange of information. It seemed like we rapidly knew of any new methodology that was worked out, whether it was a stable trap, a bait can, or the newly developed miniature CDC light traps (which replaced the large ones that we'd been using). Then we added carbon dioxide as an attractant to light traps instead of just using the light. We showed that if you took the light out of the trap and just used the carbon dioxide, you didn't get all the junk insects that were attracted to light. You also didn't get male mosquitoes, you didn't get moths, you didn't get beetles. All that information on

new techniques got exchanged very, very rapidly in the newsletter, by reports, and by publications.

Hughes: Nobody worried about trying to publish the information before it appeared in the newsletter?

Reeves: The newsletter and reports were considered prepublications and could not be used as a reference. I never worried about priority of publication. I went annually as a consultant to the CDC programs as they moved from Logan, Utah, to Greeley and then Fort Collins, Colorado, and I didn't hold back any information on what we were doing or why. My attitude was that there was too much to be done. As a matter of fact, I probably was guilty at times of feeding ideas to people so we wouldn't have to do them ourselves.

As an example of a problem I referred information on, we obtained information on the temperatures required for St. Louis and western virus to grow in mosquitoes. Archie Hess, Charlie Cherubin, and Lou LaMotte shortly thereafter studied the impact of temperature on the occurrence of epidemics of St. Louis and western equine encephalitis. They did this on a national basis to determine if temperature affected where these epidemics occurred. Their very interesting finding was that St. Louis virus was a hot-climate virus occurring in the southern part of the United States, and western was a virus that occurred in cooler northern climates. Our publications didn't make much of a ripple at that time, but as we now get into concerns about global warming, it is turning out to be extremely important. It's getting publicity now. The three of them are now retired or out of the encephalitis business, but when we are fishing or at a national meeting, they get a big kick out of the fact that I'm currently referring to their article. There was a lot of exchange.

Hughes: Which would have been more difficult without the committee, would it not?

Reeves: Oh, yes, I think the committee had a lot to do with it. Exchange also occurred because of the collaboration that we'd had for so many years with the Centers for Disease Control, and meetings were held to be sure we talked to each other.

Hughes: Is a high degree of information exchange still fairly common?

Reeves: I'd say it has been in arbovirology from the time the American Committee on Arthropod-borne Viruses was formed and became active in the sixties up until the current day. There has been extensive collaboration and exchange of information in many areas. The American Committee on Arthropod-borne Viruses now holds meetings in association with the American Society of Tropical Medicine and



Hygiene. Many of these meetings provide informal exchanges of information and are not published. I believe that exchange of information and collaboration have been the secret of success in this field. We didn't have enough resources to do all we did any other way.

### Finding Support for Control Programs##

Hughes: There's a paradox. The more successful your surveillance and control systems are, the less evidence of encephalitis cases. The lower the incidence of encephalitis, the more difficult it is to get funding and to get young scientists to come into the field, and I'm sure there are other things that I haven't thought of. Do you have any comments?

Reeves: You're darn right. All I'm doing is sitting here nodding my head "Yes" at every one of those points. [laughter] So you're extremely intelligent about this whole affair.

It's a problem: if you're successful, then you may be out of business. But this is true in many public health programs. If you control polio or measles, people will say, "There's no polio or measles. Why do I have to be vaccinated?" You find out why when you stop vaccinating and an epidemic occurs.

In vector control, if you're successful, as we've been here, then they say, "Why do we need mosquito control anymore? There's no problem. I don't see mosquitoes." At the same time, people who live in rural areas may be bitten by a lot of mosquitoes. In '58, we had the nurses do a survey of the number of mosquito bites they could observe on children's skin at clinics held by the Kern County Health Department. It wasn't unusual for them to report a hundred mosquito bites a night in infants from rural Kern County. Now, that's a lot of mosquito bites, and it was a year when we predicted an epidemic.

I've been out in the field where we were turning mosquitoes loose for a mark-release study, and those mosquitoes were hungry. We had a foreman for a ranch standing there watching the activity with a great deal of interest. He said, "We really don't have any problems with mosquitoes out here." Meanwhile, he's just slapping himself. But he's killing my mosquitoes, and I'm trying to get him to go away so that he'll stop killing my mosquitoes. He doesn't think there's any problem. But you get a mosquito control program in there, and pretty soon one mosquito biting him or his wife is all that's necessary for him to pick up the phone and call

the mosquito abatement and say, "The mosquitoes are terrible out here." Or it doesn't even have to be a mosquito. It can be some insect that doesn't even bite people. They see something flying around, and they call up and complain. Well, when you're paying for a service, a few mosquitoes are an aggravation. When you're not paying anything, you may choose to ignore them and live with them.

Hughes: How do you go to the state legislature to try to get more money for research and better mosquito control when you can't pose them with a big problem?

Reeves: Well, you try to convince them that it's in the best interest of the state in the long term that you continue to prevent these diseases, and that the infections are still here in the mosquito-bird cycles, so they can become epidemic again.

Hughes: Has anybody listened to you?

Reeves: I don't go to the state legislature and do this anymore. We try to operate through the local mosquito abatement districts and their statewide association that represents the people out there. As long as the population is supportive of the mosquito control program and wants that comfort and freedom from the disease, those people have a political voice. Each mosquito abatement district has a board of trustees which are local citizens. These are usually people who are very savvy politically. These are people who have immediate contacts with state legislators and can convince them that it's in the interest of their constituency to maintain this sort of program. The California Mosquito and Vector Control Association [CMVCA] has individual managers appointed to maintain contacts in Sacramento. William Hazeltine and John Combs have been very successful in this role.

Hughes: You have had that kind of advocate?

Reeves: Yes. The CMVCA has just hired a full-time executive secretary who's a very competent person on mosquito and disease control. He's Dr. Donald Eliason, who was with the CDC for twenty-some years as a troubleshooter nationwide on epidemics and their control. One of his primary jobs is to work with the lobbyist which the CMVCA hired to represent their interests in Sacramento.

In this instance, the university may be the weak link, because a university person like myself is not supposed to go to Sacramento as a state employee and lobby for research money for the university; that is a responsibility of the president and chancellor's office. They make the decisions on where the stress



is to be made for funding within the university, and they may not give us a high priority and frequently do not.

Hughes: That didn't used to be the case, did it?

Reeves: Not when there were problems with encephalitis epidemics. There has to be a problem that they can see is bigger than some of the other problems they're facing. The only way that a person in the State Department of Health Services or in the university can really get involved in legislative hearings is by invitation.

Hughes: Still, a good lobbyist should know where his experts are.

Reeves: Yes, the state legislature has staff that represents different fields of expertise. They used to have a physician who advised them on public health matters, not as a representative of outside interests but to represent them. That person would tell them whom to call in. Dr. Paul O'Rourke was the person in that position for a number of years, and he was a former student of mine. Some days it wasn't too handy for me. I'd get calls from him to testify in Sacramento when I didn't want to be called. Well, it's a network.

Hughes: And a competitive one. Is funding for AIDS a competing factor?

Reeves: Sure, it's a major competing factor, especially when the state budget is under stress. When the Speaker of the House, Willie Brown, has AIDS research as one of his primary projects, it's very competitive. It's pretty hard to get a person who represents the city of San Francisco to become very excited about mosquito-borne viruses. The power structure that Brown controls may not be interested in a disease that only occurs in the Central Valley but never has been in the Bay Area.

The CMVCA currently has proposed legislation in Sacramento to increase the amount of money available to the University of California for mosquito research. The amount of money they had for that purpose was less than \$500,000 a year, which was allocated out of the president's office and originally had been successfully lobbied by the CMVCA. That money hasn't increased at all over a period of years; all it's done is decrease in value with the decreased value of the dollar. The proposed bill was to increase that to over a million dollars a year. Then the university got involved in the bills to increase the budget in the university for pest management, which would include agricultural pests, public health pests, etc.

With the current budget problems, all of those proposals have been shelved. It's a dead issue as far as any lobbying is concerned. It's not going to be funded at a higher level, it's



not going to be a high priority area. We're competing with schools, libraries, whatever. Five positions were added to the budget of the State Department of Health Services two years ago for a surveillance program on Lyme disease. These positions were cut out of the budget for this past year in the interest of economy.

So that's the challenge facing people concerned with vector and disease control. At the CMVCA meeting in January 1992, I threw that challenge in their face:<sup>1</sup> "You're the people who represent the consumers, you're the people who represent these projects, and if you can't get them funded, nobody can. You're the only advocate." In fact, they're not the only advocates if there are epidemics. In that case, the county health departments, medical societies, and chambers of commerce also become big and very powerful advocates.

#### More on Virus Overwintering

##### Research in the Yakima Valley, 1942

Hughes: Shall we move on to overwintering?

Reeves: We came to realize the problem of virus overwintering way back in 1941. We found that *Culex tarsalis* was the vector of encephalitis in Yakima during the summertime. One day we were discussing what problems we were faced with, and Dr. Hammon said, "We don't have any idea of how this virus gets through the winter, do we?" I said, "No. We know it's there in the summer, but how it gets through the winter, we don't know." He said, "Well, you're the entomologist. How does it get through the winter?" I said, "This mosquito overwinters as adult females, so the virus must be living in them." He said, "Then you get up to Yakima and prove it."

So I took off in January 1942, a boy who was born and raised in southern California. I got to Yakima on a Greyhound bus at one o'clock in the morning, wearing what I thought were very warm clothes. I stepped off the bus and onto a sheet of ice--which I'd never stepped on before--my feet went out from underneath me, and

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<sup>1</sup>W. C. Reeves. Perspectives on mosquito research by the University of California--past, present, and future. 1992. In press Proceedings and Papers, 60th Ann. Conf. Calif. Mosq. Vector Control Assoc.

I went flat on my face in a snowbank and got out very disabused about the whole thing. [laughter] As a matter of fact, I guess I was probably cursing violently.

I got into a hotel and went to bed. I woke up in the morning and I looked out the window, and there was a J. C. Penney's store right across the street. It wasn't open yet, so I stayed in bed until it opened. I went over and bought long-handled underwear, mittens, ear flaps, and everything else. Then I went out in Yakima in the middle of winter to collect the overwintering mosquitoes and isolate virus from them. Everybody knew I was crazy, and they weren't very far off.

I had no idea where to start looking. I went off to where I had collected them in the summertime. The local health department loaned me a car, and I'd never driven a car on ice before. They didn't put chains on them; if you lived up there, you didn't use chains. I didn't know how to drive a car on ice, so I came to the first stop light, put my foot on the brakes, and went looping through the intersection broadside. I didn't have any bad accidents.

I'd go out in the field and get to where I knew a farm was; but it was a quarter of a mile back from the road, and I'd park the car and prepare to walk in. So I'd get my mosquito cage, my mouth aspirator, and my flashlight and walk into the place. They could see me coming up this snowy lane, and they didn't know who in the hell this was walking across their fields carrying this funny equipment. When I got there, they went, "Good, God, come on in, Doc. My gosh, you must be freezing." I said, "Yes." They said, "What are you out here for?" I said, "To collect mosquitoes." Well, then they knew I was crazy. [laughter] They said, "Okay, fine, fine. Why don't you sit down and have a cup of coffee?" and they'd try to talk me out of this nonsense.

So then I'd go out to the barn where I'd seen a lot of mosquitoes in the summer, and there would be hoarfrost a half an inch thick on the wall. I'd look and look for mosquitoes, and I couldn't find any. Then I discovered they had root cellars where they stored vegetables so they wouldn't freeze. I went down in the root cellars and basements, and there were the mosquitoes sitting around down there, just hibernating through the winter.

The local newspaper picked up on this; they thought this was great. So they ran a thermometer on the front page of the Yakima daily newspaper on how many mosquitoes I'd collected in the last twenty-four hours. I got up to several thousand mosquitoes. But of all those mosquitoes I collected, only eighteen were *Culex*

*tarsalis*, and I didn't get any virus out of them. The *Anopheles* and the other *Culex* I collected had no virus in them either.

So that was a dud, and I called Hammon to tell him how miserable I was. He said, "Go look in the sewers. That's where you have to look; they're in the sewers." I said, "Okay, I'll go look in the sewers." So I went back to the health department and told the sanitarian that Dr. Hammon said I had to look in the sewers, and he said, "You're too big." They didn't have any big walk-in sewers. Hammon was thinking of the sewers in Pittsburgh, where people could walk in. I called up Hammon again, and I said, "If you want to look in the sewers, you come up here. You're skinnier than I am." To make a long story short, that didn't pay off.

### Later Research on Overwintering

Reeves: It wasn't until the sixties that we really started trying to find out how this virus got through the winter in Kern County. Again we established a wintertime program of intensive collecting of mosquitoes, and I mean intensive. It's hard work, because it's cold and foggy in Kern County in January. It's just a miserable place to be. You would go out where you knew there were a lot of mosquitoes in the summer, and you might spend the whole day and get a couple or ten female mosquitoes. If you got twenty mosquitoes, you'd had a big day. But if you didn't do that, you had no specimen. You just kept working and working and taking everybody you could get to go out and collect mosquitoes.

Hughes: You were using the light trap?

Reeves: They won't go to a light trap when it's cold. The female mosquitoes are inactive and the males all die. These mosquitoes were in what we call diapause and you would call hibernation. Diapause means that they're completely turned off. When the light cycle gets down to a certain critical level of the right number of hours of dark and light, it triggers them in some way; it changes their hormones and everything else in them. They go into diapause, and they become completely inactive in the sense that they don't take blood meals. They move a little bit on a warm day, but they're mostly just sitting out there. Their metabolism is very low because the temperatures are very low. They go into diapause in mid-October, and in Kern County they don't come out of it until mid-January. If you go further east to Colorado, where it's much colder, they won't come out of diapause until March, April, or May. In Kern County, by mid-January they've used up all



their fat reserves. If they don't come out then and get a blood meal, and probably get sugar, they're going to die. So you can almost set your clock by it. By about January 15 to 17, every one of those female mosquitoes will come out and look for a blood meal. There's not a lot of them, but they're all coming out and feeding.

By the end of January any female you collect has taken a blood meal. The ones that went into diapause had never taken a blood meal and they're not autogenous, so you have a marker on these mosquitoes. You can take their ovaries out, and you can say, "This mosquito has never had a blood meal." That population is found from mid-November to mid-January. As a rule, any that have taken a blood meal will die. All of these females are inseminated by males before they go into diapause.

We've shown this experimentally. We've put mosquitoes that had a history of blood feeding or not into cartons and put them outdoors. The ones that died had taken a blood meal and laid eggs. These were not the ones that we found in nature in mid-winter. The ones in nature that survived had not taken a blood meal or oviposited, they had not been autogenous, and they were completely virgin females, except they had mated. They weren't virgins in that sense, as they all had sperm in their spermathecas. They were all inseminated; they all had mated before winter. There were no males in the winter collections, as they all died in the wintertime. For practical purposes, there were no larvae, pupae, or males. There was just one cohort of female mosquitoes living through from mid-November into January, and they all fed.

In January and February it takes a long time for their eggs to develop, maybe even a month because it's so cold, and then they all go out and lay their eggs and, if they survive, feed on blood again. They can live a long time in this period of low temperatures. They may feed a couple of times on blood and lay eggs.

Meanwhile, we were collecting these overwintering mosquitoes and testing them for virus. We did not get any virus during November and December. However, after mid-January we started getting western virus out of these mosquitoes, and there had been none there before. That was a major discovery,<sup>1</sup> as virus was there in mid-winter. We didn't get many St. Louis virus

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<sup>1</sup>W. C. Reeves, R. E. Bellamy and R. P. Scrivani. Relationships of mosquito vectors to winter survival of encephalitis viruses. 1. Under natural conditions. Amer. J. Trop. Med. Hyg. 67: 78-89, 1958.

isolations in this period. When we got into the spring and then to summer, western virus had built up to high levels. The question was where it was during the mid-winter, when the mosquitoes were not infected.

Now, at the same time we were doing other studies with field-collected mosquitoes. We took some additional mosquitoes that were reared in the laboratory or collected in the field in the fall and fed them on viremic chicks and then put them in ice cream cartons with little screens on the top. We also put some cotton pads soaked with sugar water on top so they would have some energy source if they wanted it. We put these cartons outside in cellars that we had dug in the yard of the lab. We watched these infected mosquitoes, and if we kept them alive they remained infected the whole winter. The difficulty was that if we let them lay their eggs, they would die. If we took mosquitoes from nature that had not taken a blood meal and oviposited, they lived very well but were not infected.

When we took the blood from mosquitoes that were collected from the field in January or February and identified it, they all had fed on the common birds in the environment: the house sparrows, house finches, and other dickey birds.

In summary, we showed experimentally we could infect mosquitoes, and they would carry virus through the winter,<sup>1</sup> but we couldn't find that sort of mosquito in nature. We could find virus in mosquitoes in mid-winter when they started feeding on birds. We had a gap of two months in the winter when there was no virus in the mosquitoes. Now, that posed some real problems for us.

I told you earlier of our attempts to find virus in birds and mammals during the winter.

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Reeves: However, we never were able to infect a mosquito on one of the experimentally infected birds, yet we were able to show that birds would maintain virus as a chronic infection for up to a year. We considered doing studies to determine if malaria infections in the birds may suppress their antibodies and make them immunologically incompetent, so they could release the virus and there would not be an antibody barrier to prevent infection of a mosquito vector.

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<sup>1</sup>R. E. Bellamy, W. C. Reeves and R. P. Scrivani. Experimental cyclic transmission of western equine encephalitis virus in chickens and *Culex tarsalis* through a year. Am. J. Epidemiol. 85:282-296, 1967.



The National Institutes of Health didn't fund that project, but it could be an answer to the question. It looks like a chronic infection in a bird is the most logical way to extend the summer cycle through the winter into the following year. We know that thousands of birds are infected every year, and a large number survive to the following year. We know that malaria infections are common in these birds, and that *Culex tarsalis* feeds on the birds as a preferred source of blood.

We once had a theory that because the malaria parasite and the virus both were in the blood, there might be a wedding between these two and the virus would get into the malaria parasite, which we know causes a chronic relapsing infection in the birds. When the birds that had the malaria infection would relapse with the malaria in the spring, which human malaria does, that would release virus that was inside the parasite. Or the *Culex tarsalis* vector might pick up a malaria parasite which was carrying the virus. Then as the malaria parasite developed, the virus would be released. All the evidence was against that, and we did a lot of work on it.

People say we don't know how western and St. Louis virus get through the winter. My response to that is, we know they do survive the winter.<sup>1</sup> We've shown experimentally that they are there and that they can be in mosquitoes and in birds. The person who wants to have more evidence than that is going to have to go out and do it himself, unless we get a lot more money to do research.

#### Other Ideas about the Introduction of Encephalitis Virus

Hughes: Is it not reasonable to think that the virus is reintroduced afresh every season?

Reeves: Some people have said that it is introduced annually, but the first question, then, is where is it introduced from? These same people say that, well, some of these birds go way down into South America and Central America, and then they fly back up and bring the viruses with them. The difficulty with that is that no one's ever been able to prove it. A lot of people have spent a lot of

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<sup>1</sup>W. C. Reeves. Overwintering of arboviruses. In W. C. Reeves, Epidemiology and Control of Mosquito-Borne Arboviruses in California, 1943-1987. Calif. Mosq. Vector Control Assoc., Sacramento CA. pp. 357-382. 1990.



money chasing birds all up and down the Mississippi flyway, the Atlantic seaboard, and the Pacific coast, trying to prove this, and they can't do it. As a matter of fact, what evidence they have found looked like any virus movement might be from the north to the south, which doesn't make any sense at all.

We do know that for almost thirty years, without a single break, we had western and St. Louis virus all over Kern County and other areas of the Central Valley. Viruses appeared all over Kern County in the spring. It wasn't just at a single spot and then spread to other spots. In a single week it appeared here, here, and here, and these positive sites might be fifty miles apart. It's a little hard to believe that virus was being introduced effectively at one time and in this pattern.

Now, both western and St. Louis virus, for practical purposes, have disappeared from Kern County. They are still in southern California. But when virus is detected in the San Joaquin Valley, it usually appears in the mid-summer period. It's not a time period when birds are flying from the south to the north. Nothing fits.

People did some work in southern California and said, "The virus doesn't persist here because we cannot get it except in the summertime in the Coachella-Imperial Valley, so it's being introduced every year from Mexico." That's fine, and the timing looked right, except they didn't know where it was in Mexico. I'll tell you, the area of Mexico that's adjacent to the Imperial-Coachella Valley is not exactly a haven; there is a lot of arid desert down through that country. It's rough territory. In addition, they were not making an intensive search for viruses year round.

Recently we have gone to the Coachella-Imperial Valley and worked there, and we find virus in mosquitoes, practically speaking, every month of the year. The mosquitoes down there don't go into a winter diapause. Sentinel chickens convert serologically in midwinter. So it looks to us like western and St. Louis virus are active every year in that area and don't have to be reintroduced. Whether that's the area for reintroduction into California's Central Valley, I don't know. But if it is, it's some bird that flies the wrong direction at the wrong time of year--that is, from south to north in late or midsummer.

So we are thinking about other alternative hypotheses. They're strictly hypotheses, and they're pretty wild. One is that the transportation system that functions between that southern area into the southern metropolitan area and the Central Valley is very intensive. Trucking moves agricultural produce from down

there into Los Angeles, the Central Valley, and San Francisco. All you have to do is watch them go by on Highway 5 or 99. There's a fantastic amount of traffic coming from those areas, and mosquitoes are very good passengers. So I think if there is virus activity in the Coachella-Imperial Valley, it has a good chance of being moved north by "hitchhiking" mosquitoes any time of year.

Hughes: Are you actually working on this?

Reeves: I don't know how to work on it. It's an hypothesis; I don't hesitate to talk about it, but I don't know how to go about proving it without getting the truckers into a big mess politically, unionwise and any otherwise. Those people are fed up enough now with the border quarantine stations, weighing stations, and so on they must go through. I have thought seriously of marking a hell of a lot of mosquitoes in the Coachella-Imperial Valley and seeing if I could catch them in Kern County at truck stops, but I haven't done it yet. It seems impractical.

The other thing we're watching very closely is to see if mosquito species unique to the southern area appear in the Central Valley. One species of mosquito is unique to that area and is a very common pest mosquito down there. If we catch a couple of those in light traps in Kern County or elsewhere in the valley, I'd be in business, but they haven't shown up yet. The other problem is, if they do show up, will they be recognized? If a person is going through thousands of mosquitoes identifying them and getting sort of droopy-eyed, and some strange mosquito shows up, it's liable to be called anything. I don't think that would happen, because we certainly have recognized other species when they have been introduced, but not till they got established and were common.

Hughes: Is it possible that virus survives in mosquitoes by infected mosquitoes transmitting it through their eggs?

Reeves: That's a possibility that is not unique to our viruses. It has been found that some of the mosquito-borne viruses can be transmitted through the eggs. Certainly, there's no question that California encephalitis virus is very effectively transmitted by infected females to their progeny. It undoubtedly gets through the winter that way, there's no question about it. We can isolate California virus in the spring from larvae or from adults reared from larvae and pupae collected in the field. If we select the females carefully in experiments, we can get females that transmit

the virus to 90 percent of their progeny. That is a very efficient system.'

But with western virus we've never been able to get transovarian transmission. In St. Louis we can get it, but we have to reduce the temperatures of the females down to about 18° C in order to have it be effective, and that doesn't make any sense to us. Don't ask me why. [laughs] If things don't make sense, don't ask me why.

There is the same problem with a whole range of new viruses that we and the State Health Department people have found in California in mosquitoes. Some of these get through the winter very, very efficiently. Turlock virus is one example, but we can't transmit it transovarially.

### Mathematical Models

[Interview 7: March 13, 1991]##

Hughes: Can you give me a little background on the use of mathematical models in virology?

Reeves: Yes. The real problem in developing statistical or mathematical models on virus diseases is that when you start talking about the life history of viruses, animal hosts, and all the different aspects of a mosquito-borne virus, there really are not enough data available in each of the compartments to construct the model. You must have information--vector biology, virus activity, virus characteristics, risk of human infection, and the degree to which different animals are involved. Tied in with this is detailed climatological data. You find there isn't the detail that you need to construct an exact model, because that requires a fantastic amount of information.

A model of any of the arboviruses and other zoonotic infections is particularly difficult because they're so complex. If you are dealing with the transmission of measles directly from person to person, with almost every case being clinically observable, it's relatively easy to include various aspects of the virus's history in the susceptible human host and the role of

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<sup>1</sup>M. J. Turell, J. L. Hardy, W. C. Reeves. Stabilized infection of California encephalitis virus in *Aedes dorsalis*, and its implications for viral maintenance in nature. Am. J. Trop. Med. Hyg. 31:1252-1259. 1982.



immunes in blocking transmission. If you can get all of these variables in, you can develop a very nice model or projection of what's going to happen, and it's been done extensively. You can do the same for polio, even though in polio a large proportion of infections are not clinical cases but are inapparent infections; but you know how frequent they are because you can determine it by serological surveys.

But when you get into something where you've got vectors, animal hosts, man, and environment, and all are playing such a key role, with man being an accident on one side of the cycle and not really important as a source of mosquito infection, it gets very difficult to have enough detail.

So we'll have a student come along, and Dr. Tom Moon was an example of this. He was a student in biostatistics, and he sat in a class where I described for three hours the variables that affect the transmission of western equine encephalitis. He came up to me at the end of that lecture all excited, because he'd been looking for a thesis problem. He said, "Could I do this as a thesis problem? Could I model this?" I said, "What sort of a model do you want to do?"

He wanted to do pretty much what we call a deterministic model; it's not one that has a lot of parameters and variables. But it would have enough straightforward information that he could take our knowledge of the mosquito, starting with when it finishes overwintering, which is a nice starting point. Only the females live through the winter, and then they lay eggs and produce what we call a first  $F_1$  generation. Statistically, you can then develop the model based on our knowledge of how that population grows as it progresses through succeeding generations into the summertime. You can then fit in our knowledge of virus infection in the mosquitoes. Without getting into unnecessary detail about where the mosquitoes are getting their infection--what species of bird and how many birds there have to be--you can deal almost solely with the mosquito and the virus as your issues and take them forward through time through a summer.

Then Tom could calculate what would happen to the mosquito population if you did a control program on the mosquitoes early in the spring when the population was just getting started. You could project what would happen to that population by the middle of the summer, how many would be left if you had a good, early control program. Indeed, as you might expect, if you could kill a high proportion of the overwintering adult population, you wouldn't have many left in the summer. In a mathematical model this looks very exciting because it's easy. Then you tell the mosquito control people, "Look, stop trying to kill mosquitoes

when there are a lot of them and they're bothering people and people are calling you on the telephone complaining. Don't spend your money in the middle of the summer when there's a real problem and a lot of virus is being transmitted. Do your control program in the early spring when there are only a few of these mosquitoes, and it will have a major effect later."

This makes a very nice model. Tom did it,<sup>1</sup> and people read it with a great deal of interest. The real problem was to sell that model to a mosquito control district. If they spend their money in the wintertime when it's very hard to find these mosquitoes--there's one here, there's one there, and they haven't built up in population and nobody's complaining--there is little interest. Then if this new program doesn't work, they have a real problem because their jobs are on the line. If they're successful, they still will be criticized, because the question will be: why are we spending all this money on mosquito control when there's no problem?

So it's very difficult to convince the control people that it's a scientific probability that they are going to be very successful if they follow your model. I don't know of any mosquito abatement district that puts a major part of their program into trying to control the mosquitoes that have become active just after the overwintering period, or that is trying to find and kill the female mosquitoes that live through the winter. A real problem is that you can't find them. I told you earlier that when we were doing overwintering studies of virus, we might go out and spend a whole day and collect five mosquitoes and eventually maybe find virus in them, maybe, but that's not a practical approach for a mosquito control district.

They usually don't start their mosquito control program until late March or early April when about the third generation has evolved from the overwintering mosquitoes. Their control program, if it's very extensive, is successful, but the model says they ought to be starting earlier to get a maximum effect.

Hughes: Is it common to use mathematical models in attempts to control different viruses?

Reeves: No, there's been very little of this done. I don't even know of any mathematical model for control of dengue fever or yellow fever, the really big epidemic diseases.

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<sup>1</sup>T. E. Moon. A statistical model of the dynamics of a mosquito vector (*Culex tarsalis*) population. 1926. Biometrics 32: 355-368.



Hughes: Now, why is that? Because there's not the data, or because there's just not the money and the personnel?

Reeves: Both. There isn't a lot of money available for development of this approach, plus the fact is that people aren't sure that that's where they want to put their money. They'd rather put it into an actual control program. Now, I think that such models ought to be attempted, and I think that the necessary data should be collected that would allow the models to be developed. The reason for this is that we really find out what we don't know when a student or statistician tells you, "I want to develop a model on this." Then they need certain data, and you soon learn what data you don't have. This is what always comes out of attempts to model anything. When there is no data they have to make assumptions to fill the gaps in knowledge. Now, when you make an assumption, it can be right or it can be wrong; you can't be sure your assumptions are right.

Hughes: When Tom Moon was setting up his mathematical model, did he indeed find that there were gaps in the data?

Reeves: Either he found it or I recognized it when he asked me the questions. I'll give you an easy example of this. We knew a lot about how many eggs a female *Culex tarsalis* lays in the summertime. We knew how many eggs an autogenous female that didn't have to have a blood meal would lay--which is comparatively few, a little over a hundred per female--and we knew that a female in the middle of the summer when it took a full blood meal might lay up to two hundred eggs. Tom wanted to start his model in January when these mosquitoes first came out of their overwintering. Now, this could be a different mosquito in the winter. We hadn't studied it during that time in detail. He says, "How many eggs does an overwintering female lay?" I say, "I don't know." He says, "That's where I'm starting my model. I have to know." I said, "Well, that's easy. We'll find out." It was in January when he asked me this question. I said, "Two weeks from now you and I are going to get in a car and go down to Bakersfield. We'll collect a bunch of overwintering females, and if they haven't taken a blood meal, we'll feed them blood. We'll get the eggs from them, and you can sit and count the eggs, and then you'll know." He said, "I'll count the eggs?" I said, "I'm not going to count them for you. You can count eggs, one, two, three, four, five, can't you?" He says, "Yes, I guess I could if I knew what they look like." I said, "You'll know what they look like."

So we collected the mosquitoes, and indeed they were just ready to take their blood meal. They always feed between the 15th and 31st of January because they have to have blood to live and



lay their eggs. He got his mosquitoes, and they laid their eggs, and he sat and counted them. And what did they do? They laid the same number of eggs as the females did that took blood in the summertime, but more eggs than autogenous females would. Obviously, you had a range of how many eggs they laid, but the average number that they laid in January was the same as in the middle of summer.

Hughes: Did that surprise you?

Reeves: I didn't know what to expect, so it didn't surprise me. Why guess when you can get the actual data? We just hadn't thought of that question before because we weren't thinking of modeling. But the modeler needs good, accurate data so he doesn't have to make an assumption.

Hughes: Your whole model would be off if you were wrong?

Reeves: It could be off pretty badly. Now, the difficulty with this is that the model, which is one of the few attempts that have been made, still doesn't answer all the questions about the very complex interrelationships of the vector, host, and virus. The reason is that it gets so complex that even with the modern computer it's not easy to put all the data together into what we call a deterministic model based on actual data.

Currently we have another graduate student, Joe Eisenberg, who has finished another statistical thesis.<sup>1</sup> He's an engineering student, and he's interested in a completely different approach to models--namely, describing what all the variables are that are going to be in the model and determining the likelihood that one variable is more important than another.

We've made all of our data available that he's asked for. When a student wants to do a model, the first thing you have to do is to make them an instant expert on everything you've learned in all of your years of research. They have to know and understand all of the variables. So Joe and I spent four or five half-days in my office, with me pounding away at him on what the variables were. Then we sent him to Bakersfield to visit with Bill Reisen and make a field trip with him so he would see how the data were gathered in the field. This answered many of the questions that he needed to answer to develop his model. Marilyn Milby provided additional data from her computer bank when Joe needed it.

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<sup>1</sup>J. N. Eisenberg. The Population Dynamics of *Culex tarsalis* in Inland Agricultural Valleys of California. 1992. Ph.D. thesis, University of California, Berkeley.

Hughes: Is the idea that if you change one of the variables, you might achieve control?

Reeves: You hope that if you change one of the variables, you will achieve control or do control in a most economical fashion. You're hoping you'll find the weak link. Moon thought he'd found the weak link by control of overwintering vector populations, as that would have the most effect on their biological potential to increase in numbers in the summer and be an effective vector.

Hughes: You mean he would decrease the number of the eggs?

Reeves: Yes, he would eliminate a sufficient number of eggs in the spring so there wouldn't be that many progeny to go forward and increase the population in the summer. I don't know what variable Joe is going to take on. He could find that the most important variable is temperature, which we cannot influence. He could find that the most important variable is the amount of water that is available. We wouldn't worry about changing water availability this year because of the drought. In some years we might want to control agricultural use of water or flooding.

Joe or future modelers may find weak links that actually have a major effect on whether the disease develops or doesn't, or infection gets to a higher level in animals. But that might not reveal a more logical way to approach control. That's the risk that you take. A major value of modeling any of these diseases or vector life histories is that it gives you an idea of what you don't know. You still have to be the judge of whether the model has any practical applications, as the modeler may not have any idea of that.

The people at UCLA, Charles E. Taylor, and at the Riverside campus, M. G. P. Georgiou, have been interested in modeling the development of genetic resistance to insecticides in vector populations and the ways in which changing the usage of insecticide might stop the pressure that maintains genetic resistance. The idea is that you could rotate through a whole series of insecticides, and if you didn't use one for two or three years, mosquitoes might become susceptible again. Obviously, they're interested in a very practical approach--how could they make insecticides continue to be effective and not encourage development of high levels of genetic resistance?

You might ask how our data help them. Well, we have an awful lot of data on the biology of *Culex tarsalis*, both general and specific. We know how populations develop, how many generations they go through in a year, and how large the populations get to

be. We make those databanks freely available to them to work into their models on the genetics of the mosquito.

Hughes: Can these models also be used to predict epidemics?

Reeves: You would hope in time that you would have enough data to be able to predict an epidemic. I would say that up to now we really are not using a model in the statistical-mathematical sense. We're using what I'd call descriptive models, and by that I mean describing the vector biology and the variables that affect virus transmission. Our surveillance program measures different variables that are readily available to us and that we think have an influence on the occurrence of human disease or transmission efficiency. We attempt to identify variables that we think can be used to decide when to intensify mosquito control. It's not a statistical-mathematical model, but you still put the number of mosquitoes that are there into use. We believe there is a critical level of vector populations where virus transmission is not effective.

Hughes: How much virus has to be detected in the natural system in mosquitoes and bird hosts before you get alarmed?

Reeves: We haven't selected a specific number or infection rate at this time. But when we find infection rates consistently exceeding 1:1,000 mosquitoes, we are very alert. Any person who deals with modeling would say, "These aren't models." So I use a term they don't use, which is "descriptive model. That's my jargon for something which will never pass as a mathematical-statistical model, but it's very useful for mosquito control districts and health departments.

Hughes: Will it pass in epidemiological circles?

Reeves: It passes in epidemiological circles. It may be criticized, but it will pass. However, it may not satisfy a statistician.

Hughes: Do you hope to get to the stage where it will?

Reeves: I hoped long ago that we could reach that point, and we haven't. I don't think that I'll be here if and when that final concise model and the data that are necessary for it will be available.

Hughes: Are viruses that predictable? What are the long-term chances of such a model being viable?

Reeves: You can't judge that until you think you have the model, test it, and find out it does or doesn't work. I think that one problem that we have now is that we can detect virus, but we're not



satisfied with the sensitivity of our detection system; that is, we really may not be identifying all the virus that's there. As long as you can't do that, people will always criticize your model.

Hughes: Also, it seems to me that to come up with a model that is going to last, you have to make the assumption, which seems very tenuous, that all the variables will remain constant.

Reeves: We know that none of the variables in our system remain constant. As a matter of fact, no variable remains constant in a biological system, and we're dealing with many variables. We're dealing with the mosquito's biology, its life history and life table. The life table, how long a mosquito lives, has a great influence. If only 5 percent of the mosquitoes live long enough to be able to transmit virus at a certain temperature, any change in that temperature is going to change that, any change is going to affect the length of the life of the mosquito and change the proportion that can transmit infection at a certain time. Now, what variable is constant?

Hughes: They're not.

Reeves: They're not. The same variables are interacting constantly.

Hughes: But within a certain range, you can build that variable into a model, can you not? I'm thinking of your emerging viruses--what if the whole system begins to take off in a different direction?

Reeves: It has, sometimes. Let me give you a simple example of this. In 1969, when we had a lot of water in California, we had the largest population of *Culex tarsalis* we'd seen in years, and we screamed, "Epidemic!" As I told you, the state gave us money for emergency control of the epidemic, and we went out and studied the results. Every prediction index that we were using was positive. We had many mosquitoes, so there should have been a lot of virus activity. And the temperatures were right; they were high. We found no western or St. Louis virus at all in the Central Valley.

What did we find had changed? Within that population of mosquitoes, autogeny rates were extremely high. That meant that most of the female mosquitoes were not taking a blood meal in order to lay their first eggs. That meant they were three to four days old before they took their first blood meal and that increased how old they would be before they could transmit a virus if they did get infected on their first blood meal. Most mosquitoes were dying before they could take two or three blood meals. Now, there were plenty of mosquitoes, because they were out there laying their hundred or so eggs each because they were

autogenous, and that produced plenty of eggs to build up a big adult population. Everything looked positive except for autogeny. There were plenty of susceptible animals out there so they could be a source of virus, but there was no virus.

Until that year we had never recognized how important autogeny was, so any model we would have built would have collapsed. We knew there was such a thing as autogeny, but we'd never been put under the pressure of understanding why something didn't happen. We had to put together sufficient knowledge to say yes, autogeny can really mess things up.

Let me give you another example. We're very used to dealing with diseases that go directly from man to man. If we have enough susceptible people in the population and we have enough infected people as sources of virus, we are going to have contact between the sources of virus and susceptibles, and there is going to be transmission and maybe an epidemic. You can build up a very nice model of how many people have to get infected before there are going to be enough who become immune to stop an epidemic.

We said originally, the more mosquitoes, the more risk of virus activity. Very simple. For other vector-borne diseases that we knew about (malaria and yellow fever), that seemed to be true. However, when we got into the viruses we work with, we found to our amazement that at the highest mosquito populations the efficiency of virus transmission decreased.

We had another student, James Olson, who didn't do statistical modeling, but he drew comparative data together in his thesis,<sup>1</sup> confirming that when a vector population got above a certain level it cut down virus transmission. That's what all the data comparing mosquito population levels in the field with virus activity showed. The same thing happened if the population of the mosquitoes didn't get high enough; it cut off transmission.

What happens is that at an intermediate population level between very few mosquitoes and too many mosquitoes, *Culex tarsalis* feeds almost entirely on birds. They are the preferred host. But when the population gets up to a certain level, the birds don't like to be fed on anymore--they're like you or me--and they fight them off. When you get to that point, the

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1J. G. Olson. The Impact of *Culex tarsalis* Population Density and Physical Environmental Factors upon Mosquito-borne Encephalitis in Humans and Equines in California. 1979. Ph.D. thesis, University of California, Berkeley.



mosquitoes are forced to go to some other animal to get their blood, but that animal may not be a good source of virus.

No one had ever thought of this before. All of our precepts and our modeling would have been wrong if we didn't know that. It seemed logical to say that if there are more mosquitoes, there ought to be more virus transmission. However, the opposite happens. This phenomenon is unique to the particular group of viruses that we work with.

To come back to your question, you can see how these variables interact so much that it makes modeling very, very difficult. We now have enough of these variables in mind and enough databanks available that there's an increasing chance that a better model can be built. I still strongly suspect that we're always going to find there are holes in our models when we test them in the field.

#### Disappearance of Western and St. Louis Encephalitis Viruses

Reeves: Then the last thing that becomes a problem in today's world is that it's very difficult to get data to build additional models, because the viruses seem to be disappearing from our environment. So when you come up with a new question about one of the variables in the model, you can't easily find an area where the virus is highly active so you can go out to gather more data. That's one of our limitations; we almost have to work in the Imperial-Coachella Valleys now to find high levels of virus activity.

Hughes: Do you have an explanation for why the viruses disappeared?

Reeves: I think mosquito control had a lot to do with it. We've had a major decrease in the mosquito populations in the southern Central Valley due to very good control programs as well as changes in water availability and use in agriculture. In the more southern Coachella-Imperial valleys, there still is not that effective a vector control program, and they have the high populations of vectors we had formerly in the Central Valley. I think it's only a matter of time until they find the resources to do a better job of control than they do now. But we're taking advantage of the fact that there are still high vector populations and virus activity down there.

But this summer (1990)--and I can't explain it at all--western equine virus completely disappeared from that area for the first time in the history of the state. This last year we had no



activity of western virus anyplace in California that I know of, and that's the first time since before I was even in the business. We'd had western virus active every year somewhere in the state. Now there still were plenty of mosquitoes down there to transmit St. Louis virus, and it was very active. There were plenty of birds in the environment to be a source of virus, and yet the western virus disappeared that year. The hypothesis could be that temperatures were too high and the mosquitoes cured themselves of infection. But that's an hypothesis. It takes a lot of experimental work to prove or disprove this. That's why this is one of the major areas we're still working on in vector competence. Western virus reappeared in 1991 and 1992.

It's extremely difficult to make sure you have a databank that will allow a very sophisticated person in modeling techniques to develop a complete model that will solve the problems of how to better control. We frequently come up with as many or more questions than we do answers. We find it extremely difficult to find any avenues of financial support for modeling. There's an amazingly negative attitude by granting sources towards modeling, as a theoretical thing doesn't necessarily provide final answers. It's not one of the most popular areas to support right now. So when we find students who can do it, we encourage them to. It's very difficult to get a faculty person in biostatistics or in the statistics department interested.

### Jerzy Neyman, Statistician

Reeves: Our first experience with this goes back more than twenty years, when Dr. Jerzy Neyman was here as head of the statistics department. He was really the father of much of statistical and mathematical modeling. He applied this approach to many things, anywhere from eclipses of the moon and astronomical problems down to nitty-gritty biological problems.

He came to me one day and said, "Reeves, I want you--." I wonder why these people with a Germanic background always call me just by my last name? Anyway, he said, "Reeves, I want you to come up and give a seminar for my faculty and my students in the statistics department." I said, "What about?" "I want you to tell them all you know about these mosquito-borne viruses." I said, "Dr. Neyman, how much time do I have?" He said, "A one-hour seminar." I said, "Dr. Neyman, I'll make a deal with you. I'll do that if you want me to, but I want you to come down to the epidemiology faculty and students, and I want you to tell them all you know about statistics in one hour." [laughter]

He said, "Don't be ridiculous. I know everything there is to know about statistics. I can't do it in a full year's course." "You get the point, Doctor," I said. "I can't tell your people all I've learned in a whole lifetime of working on these viruses in an hour's time. "Well," he says, "why don't you talk for the first hour and answer questions, and we'll decide how many sessions it takes?" I said, "Fine." He said, "I don't have to give my seminar for you, do I?" I said, "No, that's okay." I really was hoping that this powerhouse of statistical competence would tackle the problem of modeling western and St. Louis encephalitis.

I spent the first hour, and we hadn't gotten very far. I realized quickly that these people knew very little biology. When we got to viruses, it got even worse. When we got to disease, it was even more complex. These were very intelligent people when it dealt with statistics. But biology? No.

We quit after five one-hour sessions, and they said, "Okay, we think we know enough to start. Will you be available to answer questions?" I said, "Sure." So fifteen students and five faculty members were all turned loose on this problem. Every week Elizabeth Scott, who died a few years ago--she was Dr. Neyman's right-hand person--would come to me with more questions.

After about a year, Neyman called me up and said, "Reeves, we've decided this problem is far too complex. Maybe eventually when they get all the computers built on this campus we could do it, but there are too many variables to deal with. We thought this was a simple biological problem which would be a good exercise, and we've spent a year; everybody's confused and nothing works. We're going to drop the project." I said, "I'm sorry." He said, "Thank you for your time." I said, "You're very welcome."

Now, that's a long answer to your question. That was the powerhouse of statistical modeling in the United States of that day, and they couldn't handle the problem.

More on the California Encephalitis Surveillance Program##**Types of Data**

Hughes: Please describe the current surveillance program in California.

Reeves: The current program is managed by the California State Department of Health Services, in cooperation with local mosquito abatement districts and county health departments throughout the state, and we participate. They have developed a program to anticipate the likelihood of an epidemic of western or St. Louis encephalitis. In addition, the program determines the area of distribution of these viruses in the state, so that we know the so-called enzootic areas where the viruses are present in the mosquitoes and birds.

Now, what we really want to look at is to see what the likelihood of human cases is. But it's more complex than that. We start off in March, when we have a pretty fair idea of how much water is going to be available in the Central Valley or other enzootic areas, and we have to have water in order to have vectors. The first data that becomes available is through the water resources agencies of the state. That doesn't tell us conclusively whether we're going to have a problem or not, but if we don't have a lot of water, then we probably are not going to have a lot of mosquitoes and we're not going to have much virus activity.

The other thing available to us early, and again is free, are the temperature data. We know whether we've had a really warm spring or whether we've had a cold spring. Temperatures have to be fairly high for virus to get started in a series of transmissions between the vector and the birds. If temperatures stay low, we know that's going to retard virus development. At this time we haven't even started testing or looking for a virus yet.

By about the first of May the mosquitoes have gone far enough in the number of generations they've had that we're beginning to get some idea of whether *Culex tarsalis* populations are going to be high or low by May and early June. Usually if they're not developing fairly large numbers by that time period, they're not going to get to very high numbers until the late summer.

The mosquito abatement districts now get into the act. They are running mosquito light traps--which we call New Jersey light traps all over the state. These traps were developed originally



in the 1930s--There are over sixty mosquito abatement districts in the state, and each of them is running light traps. These data are sent to the State Department of Health Services Environmental Health Unit, which handles vector biology data. So now we have an idea from all of these districts of what level of *Culex tarsalis* populations are showing up in the traps.

This isn't a census of mosquitoes. It shows you when mosquito populations are very low, high, or medium. We get an idea of how the mosquito population is developing and where. This is a measure of the availability of mosquitoes and how effective the control programs are.

At this time we also have background knowledge on how much money there is in the state in the local taxes for mosquito control and what resources they have for vector control. We also know what sort of reserves they have if there is an epidemic and whether they have reserves they can use to control an epidemic.

Hughes: With the disappearance of these viruses, is it an increasing problem to get sufficient money for control?

Reeves: It has not had much effect at this stage, and this is because mosquitoes are pests as well as disease vectors. At this stage, people in the state are still conditioned to realize that mosquito-borne viruses can be an important problem.

About this time we also can begin to add into the surveillance knowledge the levels of virus activity in the field in the various regions of the state. The mosquito abatement districts and health departments have started to submit mosquito pools to the state virus laboratory, which Dr. Emmons is in charge of and which Dr. Lennette originally developed. They test these mosquito pools to see if they're infected with virus. So again, this is a very early warning system in that if we find virus in mosquitoes we know there is a chance that it may be transmitted to people. You have to have virus at a fairly high level in the mosquito population for this to happen. Data are coming in from all the districts of the state that have submitted samples. Those data all go into the system, and any virus isolations are reported by fax or by phone back to the local mosquito abatement districts and to the health departments.

At the same time, sentinel chicken flocks of twenty-five birds each have been put out. Recently we cut the size of the flocks to ten birds. But at sixty-four different locations in the state, the sentinel chickens have been put out in April. Each night they are being exposed to mosquito bites. We bleed the chickens monthly, and if they get antibodies, that means there's

been virus activity right where that chicken flock is. Those data are circulated in a weekly information letter, by fax or by telephone calls, to the district if any chickens become positive. So by early summer we know what the mosquito population is, we know the water availability, and we know the level of virus activity in the basic cycle in mosquitoes and in the birds. It isn't practical to look at wild birds at this stage, because they have to be caught, there are too many, and you don't know where they've been yesterday or where they're going to be tomorrow.

At the same time, the system is in place for veterinarians or physicians to report any suspect cases of encephalitis and to submit blood samples to the state virus laboratory to test and see if they are cases of western or St. Louis encephalitis. If cases are found, an immediate fax or telephone call goes out to the physician or veterinarian and to the health agencies and mosquito abatement districts.

Providing this network of information requires the cooperation three different groups of the State Department of Health Services: The Virus Laboratory does the laboratory tests, the infectious disease section follows up clinical cases in humans and horses, and the vector group handles mosquito population data.

#### Reports from Physicians and Veterinarians

Hughes: Which of these agencies is responsible for educating the physician or veterinarian that encephalitis is a problem and this is what you do to prevent it?

Reeves: That's probably the weakest link in the whole surveillance system. Each spring the State Viral and Rickettsial Disease Laboratory sends out an alert to physicians, veterinarians, and health departments regarding the interest in having clinical cases identified and urging that blood samples be sent in for virus tests. I suspect that many of those notices go into the circular file in the office, namely the wastepaper basket. We hope they don't. Follow-ups are sent when the weekly surveillance reports go to each local health department in the whole state. We hope again that somebody there looks at them.

The local mosquito control districts really do not have a primary responsibility for contacting physicians and veterinarians. They can if they wish, but they don't quite know where to start, and they almost have to do it through the health department; they're not really in a position to contact the



individual practicing physicians and veterinarians. Our experience in recent years has been that there can be a large number of cases of a central nervous system disease, and it can be called aseptic meningitis or it can be called encephalitis; but if the specimens do not get submitted for virus testing, we won't know what they are. In addition, there are private laboratories that now do this sort of diagnostic service, not only in California but elsewhere in the United States. The private lab may diagnose a case and send the report back to the physician, but if he doesn't officially report it, which by law he's supposed to, we never know about it or we find out about it sometime in the future.

Physician and veterinarian case reporting seems to be the weakest link in the surveillance system. Now, you might say it doesn't make any difference, because we already know whether the virus is present or not. Actually, we use our knowledge of where a virus is present to put the most pressure on to look for clinical cases in those regions. Sometimes in retrospect we even find we've had an epidemic. We had one in metropolitan Los Angeles in '84, and we had another one in Kern, Tulare, and Kings counties in '89, so the surveillance system worked. We knew the virus was there. But we also frequently know that there is virus activity, and it may be very active, in some regions, but no human cases can be associated with the information.

Hughes: What defines an epidemic?

Reeves: Having more cases than you expect in a normal year. In today's conditions in California, one or two cases of encephalitis proven to be western or St. Louis in any county is above the average. It's not really an epidemic, but it's more than we expect.

Hughes: Did the establishment of a surveillance system for encephalitis originate in California?

Reeves: The surveillance system was developed here originally. As a matter of fact, I just wrote two papers on the history of disease occurrence and current status of surveillance for the meetings of the American Mosquito and Vector Control Association in New Orleans this coming week. The surveillance system can tell you there's virus present. It's up to the local mosquito abatement districts whether they intensify their control or believe they have done good enough control earlier to try to prevent an epidemic. The problem is to keep the interest high, especially when the disease is controlled. When the disease is controlled, you have achieved your objective. However, if you don't maintain a surveillance system, you don't know whether the virus is still present in wildlife and the mosquitoes or if it is coming up to a



high level again, which it has to do before human cases will occur. You want to maintain a surveillance system to know what's going on, but if nothing's happening, it's even difficult to keep the interest up in the mosquito control districts. They always say, "Well, if we knew sooner that there was a problem, we could do more," which means you'd have to change your isolation methods and make them more sensitive, more specific, more rapid. That's another big research project, to improve the method.

Or some mosquito abatement districts and health departments want to do their own laboratory work. They think they could do it more promptly if they had their own system. It's going to cost them a lot of money to do it, and they don't understand that. By regulation you can't work with these viruses except in a biosafety level laboratory, which means you have high security and little to no risk of infection for the people who do the labwork. Developing a class 3 laboratory is not really a part of the budgeting of most health departments, so they say, "We'll just buy commercial kits, which will allow us to do diagnoses." The difficulty is that some commercial kits for diagnosis are not good, and there's no quality control on them. In addition, there are no kits for mosquito virus detection or isolations. It's a very difficult situation.

I'm very pleased with the existing surveillance system, and I say so repetitively. I think we have the best surveillance system in the world. Many other states are now doing similar types of programs but not with the same intensity that we're doing it here.

Hughes: In which states?

Reeves: Massachusetts, New Jersey, New York, Florida, Texas. Sometimes the program is for a single city, like in Houston; other times they're statewide, like here or in New Jersey. Sometimes they're just a sort of a gesture. But there are now a lot of surveillance systems in operation. They're going to devote a whole half-day of the American Mosquito Control Association meetings next week to surveillance in the various regions of the United States, and they're going to devote a half day to a review of the history of encephalitis in the various regions.

#### Role of the California Department of Health Services

Hughes: What was and is the role of the Viral and Rickettsial Disease Laboratory in the surveillance program?

Reeves: They do all the virological work that is done during the summertime. In other words, they are responsible for all the mosquito virus isolation work, say, from the first of April through the end of October. This last year they tested some 5,000 pools of mosquitoes, so over 200,000 mosquitoes were tested in the Virus Lab during 1990. They also test all of the serum samples that are collected monthly from the 64 flocks of 25 chickens each, depending on how many are alive at any one time. Last summer they tested over 7,000 serum samples to see if the chickens had developed antibodies. They are prepared and ready to do all the diagnostic serological tests on any human or horse encephalitis cases that occur, and they do it for free. So they're doing a major job.

The vector section of the State Health Department is responsible for making sure the data on mosquito populations are collected and summarized. Also, they're responsible for getting the sentinel chicken flocks out to the mosquito abatement districts that make sure they get fed, bled, and all that sort of thing. The state buys the chickens and distributes them.

Hughes: They have sufficient funding to do all this?

Reeves: Barely. It's a fight every year. The California Mosquito and Vector Control Association contributes extra money to allow Dr. Emmons to employ additional assistants in the summer when the peak workload occurs and to pay a set fee for each test on chicken sera. The communicable disease section does the actual follow-up on the human or horse cases when they occur.

### Role of the University

Reeves: I said originally that the university also collaborates on this. The weekly records on light trap records and virus activity come across my desk and Marilyn Milby's desk, and we may look at these in a somewhat different way than other people do insofar as maybe anticipating problems or seeing whether there have been changes in patterns of virus or vector activity that we recognize because of our background.

In addition to that, we have been carrying out studies to develop new methods that are faster and more economical to detect virus in mosquitoes. We just had a student finish a doctoral

thesis on that problem.<sup>1</sup> It has promise of providing a better and more rapid system.

Our laboratory and field team also maintain surveillance on virus activity all through the winter in southern California to see if testing mosquito pools and chicken sera during the wintertime will detect virus earlier than the other state surveillance system does in the summertime. We've indeed found that we can find virus activity in the wintertime, December-January, in the Imperial-Coachella Valley. There may be virus activity there year round.

We can detect it, but it takes a lot of effort, and it's costly to do this. We haven't been able to get anything to indicate that it would add very much information to extend the surveillance system throughout the year. It would be a very expensive process. As a matter of fact, some mosquito control districts today say that once they know there's a virus active in the area, that's all they want to know. "The surveillance system has done its job. There's virus here, so we'll stop doing the surveillance, and we'll go out and do a better control program; and if nothing happens, we'll take credit for it." I don't like to do that. My reaction is to want to know at what level the virus is and what it does as the summer progresses, to evaluate whether their control effort has been effective. I'd rather say, "In spite of what we did, this happened or that happened, or it didn't happen." So we have considerable difference in the position we take.

To the mosquito control district, a single case of encephalitis is extremely important if it's western or St. Louis. In other words, they didn't prevent this case, and that's their job. From a local health department's viewpoint, they may say, "Well, yes, so there's a case of encephalitis, but look at all the other diseases we have." One case of encephalitis is not a big deal to a health department that has ten or a hundred people in their area dying of AIDS, hepatitis, or whatever disease it might be. Encephalitis is not a relatively important disease. They look on western and St. Louis encephalitis as, "Okay, so it could be important, but one case isn't important, and two cases aren't important. Maybe five cases aren't really important to us."

The physician who has a case of encephalitis may say, "What difference does it make to me? What can I do to treat this

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<sup>1</sup>Patricia A. Weber. Antigen Capture Enzyme Immunoassay for the Detection of Medically Important Arboviruses in Mosquito Pools. Ph.D. thesis. 1991. University of California, Berkeley.



patient?" There is no treatment other than good hospital care-- an analgesic, a spinal tap to relieve the pressure, respiratory support if needed, or antibiotics to prevent secondary infections. But he has no specific treatment that knowing it's St. Louis or western would allow him to do. It's important to him to have a differential diagnosis to learn it isn't some bacterial infection that he could give an antibiotic for. He may be interested because somebody else may be interested in encephalitis cases, but it doesn't really help him in his practice or help his individual patient to have a diagnosis of western or St. Louis encephalitis.

So again it's a very difficult thing to deal with diseases that don't depend upon being spread from person to person. Every physician and every health department says, "Hey, if a measles case occurs here, I've got a problem. Somebody didn't get immunized to measles, and there may be a big epidemic." So diseases that disappear due to immunization or antibiotics or whatever are a completely different game than dealing with the accidental infection of humans with western or St. Louis encephalitis viruses that produce maybe a few cases but can also become an epidemic.

### Genetics Research

#### Genetic Resistance to Insecticides

Hughes: I read that in the early 1970s you decided to reshape your research program. Maybe it's a bit artificial to put such a concrete date on it, but was it about that time that you became more interested in vector competence and genetics?

Reeves: Let's start with genetics, if we may. About that time, as we've already stated, we knew we were having real problems in controlling mosquitoes like *Culex tarsalis* because of the widespread occurrence of genetic resistance to insecticides. We couldn't use DDT or the other chlorinated hydrocarbons anymore because of widespread resistance. We couldn't use the organophosphorus compounds, like parathion or malathion, effectively because the mosquito was becoming resistant to them. Plus we also knew that all of these methods of insecticide application were becoming increasingly difficult, because not only were the mosquitoes resistant but more and more legal restrictions and public opposition were being put up against using these insecticides for vector control.

So we knew we needed to look at other methods of control. At about that time it had become extremely popular to think that you might control mosquito populations by using genetic approaches. The control program for the screwworm fly had had very wide publicity in the United States. This is a very important pest in cattle. It's a fly that lays its eggs in wounds on animals and the larvae burrow in, destroying tissue. This decreases the condition of the animal and can kill the animal. It was an important disease problem, causing millions of dollars in economic losses, particularly in Texas and that general region of the United States as well as in Mexico.

What they had found was that if they sterilized male flies by radiation or by genetic selection and then turned them loose, they would breed with the wild flies. If the colonized male flies bred with the native females before the wild males did--and the females only bred once--then the females produced sterile eggs. This was a very promising approach, and they developed a huge system to sterilize screwworms and turn them loose by the millions and millions. Amazing decreases took place in the native screwworm population, almost to the point of eradication. They even went into Mexico and released flies. Screwworms were frequently reintroduced by infested cattle being imported into the U.S. from Mexico. It was a very successful program that the U.S. Department of Agriculture was carrying out. A person by the name of Ed Knipling was the head man in this program. Interestingly enough, we had worked with him way back in the 1940s in the Yakima Valley project, as he was then head of their mosquito research program in Oregon.

Meanwhile, George Craig and his associates at Notre Dame University had gotten very, very strongly into use of genetics as a possible approach for controlling *Aedes aegypti*, the primary vector of yellow fever and dengue. They developed a large program that had many experts on mosquito genetics who developed different genetic strains of *Aedes aegypti* which, if they interbred with field mosquitoes, would make them sterile. They developed various ways to sterilize mosquitoes by transforming their chromosomes through use of different chemical agents or by radiation. They selected mosquito populations that could be maintained in the laboratory, and if you bred them with the field mosquitoes they were genetically incompatible, and the offspring would be sterile.

We thought this was a very interesting approach, and it was getting a lot of publicity. George Craig was elected to the National Academy of Sciences primarily because of these studies. It was a very popular field, and WHO was riding it very heavily.

Many people believed that genetic approaches were going to solve all the mosquito as well as screwworm problems.

### Sister Monica Asman

Reeves: We thought we'd better get onto this bandwagon, because we hadn't given it any consideration and nobody in California had. About that time in 1972, purely by coincidence, a lady whose name was Sister Monica Asman showed up at the entomology department in Berkeley. She was in the Franciscan order of the Catholic Church. She had a Ph.D. in mosquito genetics under Dr. Craig at Notre Dame and was a well-trained geneticist. She had actually done the first chromosomal translocations on *Aedes aegypti* in his laboratory for her thesis.

When she'd finished her degree, the order had moved her to a convent in Redwood City, where she was to teach biology in a high school. She had brought her *Aedes aegypti* mosquito colony with her and was still playing with it in the convent. She didn't think that was a very good place to be working with mosquitoes, so she came to the entomology department at Berkeley to see if they'd give her some space to maintain her colony and do more work. They said fine, they would without any salary, and they gave her some space.

I ran into her one day and said, "Why are you wasting your time on *Aedes aegypti*, which doesn't even occur naturally in California? Why don't you work on an important mosquito if you're here in California?" She said, "What mosquito is important in California?" I said, "Why, *Culex tarsalis*." She thought that would be interesting, and she probably was getting bored with *Aedes aegypti* anyway, so she said, "Yes. Where can I get them?" We had colonies, so we gave her a colony and she started working with it.

To make a long story short, we later put her on the payroll. One time we had an interview here with The New York Times on the various aspects of our project, including her work. This resulted in a headline coming out in The New York Times that said something like, "University of California hires Catholic sister to sterilize males." [laughter] I had a little difficulty with that, but fortunately, Monica's and the church's sense of humor took care of the problem.

Anyway, she investigated the genetics of *Culex tarsalis* by selecting various subcolonies of this mosquito that had genetic



markers on them--a red eye, black eye, or whatever might be the characteristic, much as they do with *Drosophila* fruit flies. She started treating these mosquitoes with radiation and chemicals to alter their chromosomes. She developed colonized populations of this mosquito that had what we call chromosomal translocations. When the males mated with field mosquitoes that were brought to her from Bakersfield, the vast majority of offspring were sterile, and this was very, very good. I'm really making the story brief because there are many papers that came out on this, including a summary in our monograph.<sup>1</sup>

### Field Experiments with Genetically Altered Mosquitoes

Reeves: We then decided that this development had to be taken to the field. However, we actually had erred in one respect. I began to realize that we really didn't know anything about the biology of male *Culex tarsalis*. As a matter of fact, we knew very little about any male mosquitoes. The reason was that people generally thought, "Males don't transmit viruses and males don't transmit malaria, because males don't take blood. So it's the females we're interested in," and the males were just thrown in the wastepaper basket. You'd collected thousands of them, but you didn't know what to do with them. You knew that about half the mosquitoes that hatched out would be males and half would be females. Where they mated, how and how often they mated--anything about the biology--really wasn't known. About this time Dr. Reisen was coming into the program, and we decided that we had to study the biology of male as well as female mosquitoes in the field, which hadn't been done before.

Anyway, Monica produced a lot of the genetically modified male mosquitoes that would mate with females and make them sterile. We decided, "We're going to send these males to Bakersfield and turn them loose, and all of our problems are going to be solved." At about that time we also realized that this ought to be modeled. Dr. Paul Fine was here from the London School of Tropical Medicine as a visitor for the better part of a year, and he was interested in arboviruses, malaria, and a lot of things. He was very interested in modeling, so he and Marilyn Milby, our biostatistician, developed a simulation model of what would happen if the several types of genetic abnormalities were

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<sup>1</sup>S. M. Asman, M. M. Milby, W. C. Reeves. Genetics of *Culex tarsalis*. In: Reeves, W. C., Epidemiology and Control of Mosquito-borne Arboviruses in California. Calif. 1990. Mosq. Vector Control Assoc., pp. 330-356.

introduced into a population.<sup>1</sup> The question was whether you could sterilize enough mosquitoes in the native population to start reducing those populations.

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Reeves: We could calculate how many mosquitoes were in the field population and how many males we'd have to turn loose. The key was to turn loose more genetically modified males than were in the natural population. Then they would compete effectively with the native males, and practically all the females would be sterilized. It was also convenient that some of the genetic characteristics that were being put in were transmitted by the males and would occur in the next generation of any that survived, so it would be a self-perpetuating sort of thing.

We knew we needed to turn loose, say, 20,000 mosquitoes at one particular time in the isolated little field area we had selected at Poso Creek in the foothills of Kern County. The area was separated by some miles of desert foothills from any other place that was producing mosquitoes, so we had a nicely isolated mosquito population.

Bob Nelson was in charge at Bakersfield at the beginning, and then Bill Reisen came in. They could tell us how many mosquitoes were in the area, as they were marking many mosquitoes with fluorescent dust and turning them loose in the population. The ratio of marked mosquitoes to unmarked ones in collections told us what the total population was. We could mark and turn loose either males or females. So we had the answer to the size of population, and we knew exactly how many genetically altered mosquitoes had to be turned loose to outnumber the mosquitoes that were already there.

We turned loose as many as 180,000 special laboratory reared mosquitoes, and nothing happened. We didn't really reduce the population. We knew that we had inserted the genetic trait into some of the population, because we'd reisolated it. We knew if we introduced genetically modified and field mosquitoes into a big Quonset hut that had been converted into a screened insectary, the system worked. When we mixed the two populations into a little three-square-foot cage, it worked. But out there in the field, not much happened.

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<sup>1</sup>P. E. M. Fine, M. M. Milby, W. C. Reeves. A general simulation model for genetic control of mosquito species that fluctuate markedly in population size. 1979. J. Med. Entomol. 16: 189-199.

We went on and did a number of experiments. As a matter of fact, we worked intensively for five years on this project. Meanwhile, we were making observations on the biology of male mosquitoes in the field. To make a very long story short, what we finally learned was that the genetically modified mosquitoes that we were producing in the laboratory did not mate effectively with the field population. When the males in the field were ready to mate, they created a swarm over a bush, a fencepost, a car, or something like that. They seemed to use these objects as a sort of a marker. The wild females would come into the swarm of males and mate with them.

Well, again to simplify, what we found was that the mosquitoes that we had colonized in cages and had modified didn't swarm in the same pattern as the native males. The colonized males swarmed most of the time down near the ground, and usually between two objects like two bushes or whatever.

Hughes: Why?

Reeves: They'd been adapted by selection to mate in cages. The minute you put them into a cage, you selected a small part of the population that would mate in a small, confined space. It was not practical to colonize them and keep them in a great big outdoor insectary that was twenty feet high and maybe forty feet long. Originally we didn't realize we'd done this selection. It took us a long time to determine, and it was not easy. You can't ask a mosquito what it is going to do. We had to mark and release males and then study what they were doing. Bill Reisen did an amazing job in discovering where and how this mosquito mates.

To make a long story short, he not only found out that when this mosquito was brought into the laboratory and colonized, you immediately selected a mosquito that did not swarm in the pattern of the field population, and that wasn't what you needed in the field. He also raised large numbers of mosquitoes that were collected as pupae in the field and brought into the laboratory, and he had Monica sterilize them by radiation. When these males that had not been cage adapted were turned loose without colonizing, they did fine and mated with native females and sterilized them. But that's not the way to control mosquitoes, because that means you would have to have a mosquito population in the field that was large enough to be a constant source of mosquitoes to be sterilized. You're defeated before you begin. It's not practical to approach the problem this way.

After five years we had learned a lot about the genetics of this mosquito and its mating habits. We really had written the textbook on the genetics of *Culex tarsalis*. Monica had done her



job as a laboratory geneticist very thoroughly. We had five years of effort all underwritten by research grants, only to find out that an idiosyncrasy of the mosquito defeated the system. The modeling system worked beautifully on paper, but when we modeled it we could not anticipate the mosquito's mating habits.

They found some of the same things in the control program for the screwworm fly. The flies started to come back because the wild females didn't like to mate with the laboratory-colonized males and preferred their own types. I don't know all the details, but one of the primary reasons for early failure of the screwworm control program was that there had been a genetic selection of males that the females in the wild didn't like to mate with. So they're fickle. That's the way women are, you know. I'm sorry, I shouldn't say that. [laughs]

Hughes: Be careful.

Reeves: Erase that, please. [laughter]

Our genetic research was good science, and we learned a great deal. However, it didn't solve the problem. It still has the theoretical potential to be a method to control *Culex tarsalis*, but it would require a lot of financial support and a major effort. At this particular point in time it doesn't seem feasible or practical to try to get that support. I don't think we're going to use this method to control *Culex tarsalis* in the Central Valley of California in my lifetime. We couldn't control the population in a little tiny field area in the foothills of the Sierras by this method.

There was another question you had?

### Vector Competence

Hughes: Vector competence.

Reeves: We also realized that we didn't know much about the ability of different populations of *Culex tarsalis* to be infected with and transmit a virus such as western equine or St. Louis. The question was, are all the mosquitoes in a *Culex tarsalis* population the same as far as their ability to become infected with and transmit a virus? Dr. Hardy began work to evaluate this question in the late 1960s. He took different populations of *Culex tarsalis* from the field into the laboratory, and he found a great deal of difference in their ability to become infected with

and to transmit virus.<sup>1</sup> Indeed, we found that there were field populations that were very susceptible to relatively small doses of virus and transmitted it very effectively, and others were completely resistant to that dose of virus. We even found some mosquitoes in the field that were *Culex tarsalis* by any other measure we had, but they simply were not good vectors.

Also, we were finding field areas where we had times when everything else seemed to be right for virus transmission, but it wasn't happening. In the 1940s we had said that any *Culex tarsalis* we brought into the laboratory could be infected. In the fifties and sixties we found more and more populations that were not good vectors. We started looking into how this could be explained.

These studies coincided with the time when insecticide resistance was developing in *Culex tarsalis*. So our first hypothesis was that insecticide resistance had modified the ability of these mosquitoes to be a vector. However, we soon ruled that out because studies on laboratory colonies that were insecticide resistant versus those that were insecticide susceptible didn't show parallel differences in their competence to transmit viruses.

This is when we really started studying the mechanisms that are necessary for virus to get into the mosquito's gut, attach, multiply, penetrate from the gut into the body of the mosquito, and be disseminated to the salivary glands in sufficient amount to be excreted in the salivary secretions when the mosquito fed.

It required very sophisticated virological techniques to discover how virus attached to cells in the gut and where virus multiplied in the mosquito. It was found that in some mosquitoes there was a barrier in the gut, and the virus couldn't get from the gut into the body so it could get to the salivary glands. That was what we called a gut barrier. There also seemed to be salivary gland barriers in some mosquitoes where even though the virus was in the body in large amounts, it wouldn't get into the salivary glands effectively. So a whole new study area developed on factors that affect vector competence.

We also found that these factors were genetically controlled, and colonies could be developed that were extremely susceptible or

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<sup>1</sup>J. L. Hardy, W. C. Reeves and Robert J. Sjogren. Variations in the susceptibility of field and laboratory populations of *Culex tarsalis* to experimental infection with western equine encephalomyelitis virus. Amer. J. Epid. 100: 498-505, 1976.

extremely resistant to virus infections. We also found to our amazement that mosquitoes infected with western virus, if they were held at relatively high temperatures, became infected very readily, and the virus could get through the gut into the salivary glands. But if we continued to hold them at high temperatures, some of these mosquitoes cured themselves of the infection and weren't effective vectors.

These findings became very important, because originally we had thought, when we went out in the field and tested mosquitoes for virus, that any mosquito we found infected with virus was a bad mosquito. That made sense: if they had virus in them, they were going to transmit the infection at some time. But this didn't all hold together, so we did further evaluations and found to our amazement that at any one time only about one in four of the infected mosquitoes could transmit the virus they had fed on. What it amounted to was that the non-transmitting mosquitoes had really not completed their incubation period; they just hadn't lived long enough to transmit. But they were still taking infective blood meals. Now, the alternative was that they were incompetent vectors. The virus could get into them and multiply and stay there for a while, and we could find it if we looked for it, but that didn't mean they were going to transmit it effectively.

This raised a whole new area of concern. We had to reevaluate completely what importance it had if you found a mosquito infected in nature with a virus. What was the likelihood it was ever going to be a vector? We had to study this question from the ground straight up. We hadn't stopped to think about it before, but the concept that any mosquito that was infected was a bad mosquito was dead wrong. Also, in many places we'd find a lot of virus in mosquitoes but comparatively little transmission taking place.

Hughes: Was this a new idea in biology?

Reeves: Pretty much, yes. Well, there weren't that many places where they were collecting a lot of mosquitoes, seeing that they were infected, and at the same time seeing whether they also could transmit virus. We were the first people to evaluate whether infected mosquitoes in nature could actually transmit the virus when they fed. It's a very difficult thing to evaluate, because you can't ask them what they are doing, and it isn't practical to bring large numbers of live mosquitoes into the laboratory and feed them on an animal host.

So the way we got at this was that we developed our bait can trap, which I discussed earlier, in which we could put a three-



week-old chicken and expose it overnight in the field. The mosquitoes were attracted to the chicken, and a portion of them would feed on the chicken. But some would not feed, even though they were attracted.

We showed for the first time that a host would repel the mosquitoes when they got to a certain number.<sup>1</sup> If only a few mosquitoes went in the bait can, they'd all feed, but as the numbers went up, the proportion that wouldn't feed on blood increased. The chicken was getting sick and tired of this business and fluffed up its feathers and danced around to fight the mosquitoes off. It was interesting that John Edman and Herbert Kale observed the same thing at the same time in Florida.<sup>2</sup> In one night a thousand mosquitoes might go to one chick. When you got to that number, less than 10 percent would ever succeed in getting a blood meal. So even if infected, they might not transmit.

In addition, we took the mosquitoes out of the bait can and separated them into those that had fed and those that hadn't fed. We knew the only blood source they had was the chicken in the can. So all the engorged mosquitoes were taken out and tested for virus. The unfed mosquitoes also were tested for virus. The infection rate might be the same in those two populations of mosquitoes, because having fed on that chick didn't make a difference. The chick wasn't viremic.

We took a blood sample twenty-four or forty-eight hours after the chick was fed on by mosquitoes and took a later sample for antibodies, and we could determine what proportion of chicks became infected. So we had the infection rate in the mosquitoes and the transmission rate by the same mosquito population to the chick that was a susceptible host.

This told us that a lot of mosquitoes that were infected might never transmit virus. So by taking the life table data on the mosquitoes--how long they lived, how many blood meals they'd take, their infection rate, and the transmission rate--we understood the dynamics of natural transmission. We then transferred all that information to the laboratory, and by doing

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<sup>1</sup>W. C. Reeves. Mosquito vector and vertebrate host interaction: The key to maintenance of certain arboviruses. In: The Ecology and Physiology of Parasites. A. M. Follis, ed., Univ. of Toronto Press, pp. 223-231, 1971.

<sup>2</sup>J. D. Edman, H. W. Kale. Host behavior: its influence on the feeding success of mosquitoes. *Ann. Entomol. Soc. Amer.* 64:513-516. 1971.

vector competence work we found that mosquito populations also varied in their capacity to be effective vectors. We began to work out the actual physiological and cellular basis of why mosquitoes varied in their competence. This work is still ongoing and at a very sophisticated level. We found that competence not only was a genetic trait of the mosquito but also that it was very much influenced by temperature, even to the extent that at high temperatures some mosquitoes had cured themselves of infection with western virus.

When we talked earlier about modeling, you asked, "Why can't the definitive model be built?" We didn't know the details about vector competence when we tried to build the earlier models. So a whole new field on vector competence has now developed and is being utilized by other people. This doesn't mean we were the only people doing research on vector competence. Drs. Chamberlain and Sudia at CDC did early work that also indicated some of these things were happening, but they didn't get into all the details of what happens inside the mosquito, which Drs. Hardy, Laura Kramer, Ed Houk, and the other people working in our laboratory have been able to do.

I say that vector competence is important, and now people have said we also have to use another term, vector capacity. Vector capacity is different from vector competence. Vector competence means that once the mosquito is infected, to what extent will it be a successful vector. Vector capacity covers other characteristics of the mosquito, such as its host preference. It has to prefer to feed on birds to transmit western and St. Louis virus. If it doesn't feed on birds, that decreases its vector capacity. How long it will survive and other biological variables also influence vector capacity. What we are doing is considering both the natural history of the mosquito and the inherent ability of the mosquito to be a good host to a virus.

#### Emergence and Decline of Encephalitis Viruses

Hughes: In any of this work were you concerned to answer how these viruses emerged in the 1920s and 1930s and, conversely, how they would disappear?

Reeves: I really don't understand why western and St. Louis encephalitis were not widely recognized as distinct diseases before that time. I suspect that western and St. Louis viruses were here before any of the invading Spanish or other Caucasian North Americans came in



North America. I think they undoubtedly were here when only the Indians were here and before horses were here.

Hughes: In wild birds?

Reeves: In cycles between wild birds and *Culex tarsalis*. We know *Culex tarsalis* had to have been here. The habitat was here. We know the birds were here in the river bottoms, marshes, foothills, and so on. Now, why wasn't this type of disease reported in the Jesuit and the other journals that were kept in the earlier period? Well, number one, the population of people and horses that were here was very, very small, and a lot of them were dying from a wide variety of diseases that were not identified or described in detail. Encephalitis could have been a very common infection in that small population of people and horses without even being recognized as something that was very different. In addition, a number of infections with these viruses do not result in encephalitis; they are inapparent infections. Many people or horses must be infected to result in a few cases of encephalitis. It wasn't recorded as something identifiable.

But why it wasn't recognized more in the 1920s, I don't really know. Western became recognized as an important problem in California in 1930-31 because we had epidemics of thousands of cases in horses, and the virus was isolated by Dr. K. F. Meyer and associates in 1931. The horse was a very important animal in our agricultural economy. It was literally a horsepower source. If you didn't have horses, you didn't have horsepower. We didn't have all the mechanization we have now. Encephalitis was causing thousands of deaths, so it became important.

Now, there could have been a similar disease that occurred in the earlier period--1915, 1920, something like that. It may just have been here and was not recognized or differentiated. That's perfectly possible. Again, there were plenty of diseases in our animals that were causing problems and were given names such as forage poisoning, botulism, and horse plague.

Hughes: But there were no reports of epidemics in horses?

Reeves: Dr. Meyer is the only person I know of who considered this problem. He published two papers that refer to epizootics of a similar disease as early as 1847. In 1912, an epizootic caused 35,000 deaths in an extensive area of ten states but not including California. From 1915-1920, another outbreak affected horses in California, Colorado, Oregon, Nevada, and Montana. He refers to a



number of such episodes in this early period, but no causative agent was identified until 1931.<sup>1</sup>

Hughes: Is that true of the East Coast as well?

Reeves: Same thing on the East Coast, yes. Now, it could have been that we got to a critical level of horse population where encephalitis was ready to go, and it went. The horse is not an important host as far as being a source of the mosquito infection; it's just an accidental host. I find it difficult to believe there was a change in the viruses. The western and St. Louis viruses are very stable viruses. They don't readily change genetically. They're not mutable like an influenza virus. In the time that we've been working with these viruses, they haven't changed markedly. From the 1930s to the 1990s, the viruses haven't changed in their virulence or infectivity for vectors and so on. We still have a lot of the old strains available to use for comparison.

St. Louis encephalitis also had the appearance of being a newly emerged disease. In 1931-32 there were small outbreaks in the area around Paris, Illinois, and then in 1933 there was an epidemic of a thousand human cases in St. Louis. It's hard to believe that was the first time St. Louis virus was ever in the United States or that some big change took place then, because in a few years it was found to be a very common virus over much of the western United States and later in eastern states. I can't believe the virus emerged anew.

I just wrote a paper I referred to earlier on the history of these viruses, and all I could say was that in the 1930s, these appeared to be newly emerging diseases in the sense that they weren't known. We didn't know what caused them, we didn't know where they came from, and they were suddenly recognized to have an epidemic or epizootic ability. They were just as much newly emerging diseases as AIDS or the hemorrhagic fevers are today in many areas of the world. They were newly emerging diseases in the 1930s, and that's not that long ago. I was here, not working on them at that time, but I was a few years after that.

Now we know these viruses have a very wide distribution over the western United States, and we know pretty well where they are, even into the eastern United States in the case of St. Louis. In California, both viruses for practical purposes have disappeared in recent years from the San Joaquin Valley. St. Louis has come

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<sup>1</sup>K. F. Meyer. Equine encephalomyelitis. The North American Veterinarian 14:30-48, 1933; and K. F. Meyer, Neurotropic virus infections of the horse. Proc. 5th Pacific Science Congress, pp. 2915-2925. 1933.

back to low levels in the San Joaquin Valley in recent years. You say, "Well, then you must be able to explain why the virus has disappeared."

I'd like to be able to say that it has disappeared entirely because of vector control. I can't say that. We still have very large populations of *Culex tarsalis* in parts of the Sacramento Valley. Controlling this mosquito in rice fields is a very difficult problem. You have many square miles of water. The mosquito is not too abundant when you look for it in the average rice field, but a mosquito larva or two in every square yard adds up to a lot of adults flying around. The virus has disappeared from these areas as well as from areas where vector control has reduced *Culex tarsalis* to very low numbers. In most years, both viruses have continued to be active in the Imperial-Coachella Valley almost throughout the year; again, this is an area where *Culex tarsalis* is very common.

You really must stretch your logic to imagine that it is possible to have an area of *Culex tarsalis* abundance with no virus, an area of *Culex tarsalis* rarity but still with a little virus activity, and yet another area where *Culex tarsalis* is common and both viruses are active. We have very flimsy explanations for this occurrence. When both viruses were active in the Sacramento Valley, we had places where there were just impossible numbers of *Culex tarsalis*, where there was no control, and there were hundreds, even thousands of mosquitoes per light trap night. People stayed indoors at night to keep away from them. And yet there was no or very little virus activity.

Well, the explanation we had for that at the time was that there were too many mosquitoes, so they weren't feeding frequently enough on the birds that were sources of virus to be effectively transmitting virus. I actually told mosquito control people, "When you go into one of these areas, don't just control the mosquitoes to a lower level, because you may make it a more efficient vector. It may be better to do nothing than to reduce the population partially." They didn't like that sort of information, and I may have been wrong. But the only explanation I would have now is that there were just too darned many mosquitoes out there for virus to be transmitting effectively. I don't like that explanation, as it's not true in all the areas.

In contrast, when we had St. Louis encephalitis virus activity in Kern, Kings, and Tulare counties in the last couple of years, the *Culex tarsalis* population hadn't gotten very high. Yet we had virus reactivation in areas where it hadn't been detected for some years. It looks like it may have been reintroduced, although it has now shown up for a couple of years in a row, and



that may not be reintroduction. We're also finding that St. Louis virus is active every year in the metropolitan area of Los Angeles, but not at high levels and with few or no human cases. So it's a dilemma. It's also a dilemma not to be able to answer why virus activity continues in the Imperial-Coachella Valley at a high level in *Culex tarsalis* and sentinel birds, but there are no recognized cases of encephalitis in horses or humans.

Hughes: Are you pretty sure that wild birds aren't bringing in encephalitis viruses each year from different areas of the Americas?

Reeves: You can never be sure of that, except that if you look at the history of western and St. Louis viruses in the San Joaquin-Sacramento Valley, they were there constantly for thirty years. I wrote the book on exactly when and where virus was going to appear each year. As I said previously, it didn't just appear at one spot and then spread from there. It would appear within a one-week period at a number of different places. Now, what the hell was going to carry virus into a wide variety of places within a week's time so it was easily found? It was there every year. We also isolated viruses during the wintertime from the mosquitoes, birds, or rodents.

Hughes: So it was overwintering.

Reeves: It appeared to be overwintering. For practical purposes, these viruses still are present year round at some level in the Imperial-Coachella Valley area. I don't like to say a virus is being reintroduced every year into a place when we know it is present all the time. In today's world, the Imperial-Coachella Valley area could be a source of reintroduction of virus into the Central Valley, where it has been absent for a period of time. Except it's happening at the wrong time of year as far as our detection system is concerned. The virus has been reappearing in mid to late summer. I don't know any bird species that flies north at that time; they're going south. It's the wrong time of year for any animal except people to be coming north.

Marilyn Milby likes to keep reminding me that the Central Valley aqueduct extends from northern into southern California. I say, "There was no such aqueduct until the 1960s, and it doesn't go to the Imperial-Coachella Valley." She says, "But it goes to the Los Angeles area." I say, "But I don't know any animal that carries encephalitis and swims upstream. Also, there are a few siphons to go through to get through the Tehachapi Mountains. I don't know anything that goes in that direction. The other direction, I could see that something could happen."



So you're asking an impossible question. That's why we're still doing research. That's what makes research exciting, interesting, and challenging. I think we have a lot of knowledge we didn't have before, and we still can't answer all of your questions. I can't say it's embarrassing; I find it challenging. I came into this work at a time when we didn't know the answer to hardly any of the questions. Then we felt we'd answered most of the questions, and now we find out there are still unanswered questions. We've gone the whole circle.

Now we're waiting for these diseases to reemerge. If there is a significant time before they reemerge, the physicians and the health department will say, "These are new diseases," and probably we will have forgotten everything that we knew. The viruses don't seem to have changed, the vector hasn't changed that much, except in abundance, the same birds are here--maybe not in the same exact numbers as they were before, but there are still plenty of susceptible birds. You asked the impossible question, and I didn't give you a very definitive and good answer.

#### Effect of Television and Air Conditioning on Encephalitis Incidence

Hughes: What about the effect of television and air conditioning on encephalitis?

Reeves: This is not necessarily the most popular topic to discuss as far as mosquito control agencies are concerned, but we will anyway. In 1978, Proposition 13 was enacted in California, and it cut the budget for mosquito control and most other public services in half in this state by decreasing the tax rate. Mosquito control districts suffered an unexpected and really severe cut in staff, resources, and equipment. When that happened, the *Culex tarsalis* populations started to rise, and in the early eighties virus activity had increased in the whole Central Valley area. We had western and St. Louis virus activity each year. Sentinel chickens and *Culex tarsalis* were being infected, and there were some western cases in horses but few or no cases in people. Some of the mosquito control people in the Central Valley said, "Look, Reeves, how do you explain all this? We're having trouble controlling the mosquitoes. The virus is coming back and we're yelling epidemic potential. Few to no human cases are being reported."

I had a very hard time answering that question. Now, some cases could have occurred and not been diagnosed or reported, but

there weren't any epidemics. We looked again at the vector competence of mosquitoes, and it hadn't changed. The viruses hadn't changed in virulence, they hadn't changed in other genetic traits as far as we knew. That was a real problem. I finally got tired of this question, and I'll confess that for practical purposes I sat in the corner one day, put my head down, and thought it out: "There's got to be an explanation for this. We've looked at the obvious things and can't explain what's happened."

I finally put myself in the position of a *Culex tarsalis* female that's carrying a load of virus around and wants to give it to a person.

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Reeves: I knew if this mosquito was going to feed on blood, it would start to feed a half an hour after sundown. We can set our clock by that. In the summertime we can go out in the field, and a half hour after sundown we know the male mosquitoes are going to start swarming and the females will come to mate. The females will come out from hiding and start biting. Every night the time changes by a few minutes, but we know exactly when it will be after sundown. The sun goes up and down at regular times, and that hadn't changed.

I also knew that *Culex tarsalis* was primarily going to feed out of doors. It will go into houses, but it is primarily an outdoor biter. It's going to feed on people and on birds a half hour after sundown. We had been doing studies on its feeding habits for a long time, and it hadn't changed the hour it bit. So the mosquito hasn't changed, and the virus hasn't changed. What else could have changed?

I started thinking, has anything changed about people? Now, that seemed very unrealistic; they have the same habits, both bad and good, as they always have. But I suddenly realized that to be effective, this mosquito had to bite people a half an hour after sundown. The question became, are people doing something different a half an hour after sundown now that they didn't do in the 1940s and the early fifties? All of a sudden the light came on.

Hughes: I think I know the answer. The "idiot box"?

Reeves: Yes, the idiot box. The prime time on television is exactly at that time.

In addition, when I went down to Bakersfield in the 1940s, air conditioning was almost unheard of. Then desert evaporative coolers started coming in. But it wasn't until post World War II and up in the fifties and sixties that both television and air conditioning became the rule rather than the exception in most households. [tape interruption]

I had a student named Paul Gahlinger with a Ph.D. in sociology and who had just finished an MPH [master of public health degree] with us in epidemiology. He had a time interval between graduation and when he was going to take an epidemiology faculty job at San Jose State University, so I gave him the job of doing a survey on air conditioning and television in Kern County to obtain data to show that television and air conditioning might have a protective effect against mosquito bites and encephalitis. He got a bunch of nurses from the health department to work with him on a volunteer basis. They did telephone surveys of people randomly selected from the telephone book and asked them questions about their use of television and air conditioning and its relationships to mosquito attacks.

Very interesting data came out. People had indeed changed over the years in how much they used television and air conditioning. Many households had as many as five television sets, and almost everybody had at least one. Economic status had no influence; everybody had television. They also all had air conditioning. There was a beautiful fit of the time period when air conditioning and television came into use and the decrease of encephalitis cases in people but not in horses.

Hughes: That makes sense, doesn't it?

Reeves: Horses don't have television and air conditioning. [laughter] Now, that doesn't mean that air conditioning and television controlled encephalitis, but virus activity rose in vectors and avian hosts during this time period, and this new social factor came into being.

When we went down to Los Angeles during the epidemic in 1984, it turned out that air conditioning was not the rule in Los Angeles. A lot of people still spent time out of doors in the evening in the yard, walking and things like that. Some even had their television sets on the outdoor patio.

The mosquito control districts would rather take the credit for the decline in encephalitis cases, and I don't blame them. I think they've done an excellent job of mosquito control in most of the areas where there are control districts. However, it also is an example of how social changes that we're not really conscious



of and that are not done to control disease can have a major impact on the occurrence of disease. There have been several surveys by the Centers for Disease Control during St. Louis encephalitis epidemics in Texas and other places in recent years, and they find exactly the same thing. Air conditioning, television, and improved housing may be protection against encephalitis infection.

Hughes: Do the abatement districts seriously protest this finding?

Reeves: They don't protest it. After all, it's their job to prevent encephalitis, and any help is welcome. But they're not sure how much reliance should be put on television and air conditioning compared to what they're doing with vector control. I'm not sure either, and that's not my job. But I think they should know about it. As a matter of fact, they benefit by the effect of television and air conditioning because it reduces the risk of people to exposure to vectors, which means the job of protecting people becomes somewhat easier.

#### Encephalitis Research in Southern California

Hughes: Tell me about hot spots in the Imperial and Coachella Valleys.

Reeves: As I have already said, when we started working in the Central Valley it seemed we had both western and St. Louis viruses everywhere. We had *Culex tarsalis*, any numbers we wanted, and that was in both the San Joaquin and the Sacramento Valley. Each year as we did our research and got more information, the mosquito control programs extended out further and further into rural areas. Areas that had been very convenient for research and were very near our laboratories suddenly were no good for virus studies. It's pretty hard to study viruses and the association of vectors and birds if you have very low or no virus activity.

It was about this time that we became involved in studies in southern California, because they'd had a small outbreak of St. Louis encephalitis (twenty-six cases) in 1984 in the metropolitan area. This was a new area where there had not been any recognized problem before. So we started working in the area to see where the virus might have come from. There was no evidence that the virus had been active there in earlier years during the times when there had been a lot of virus activity in the Central Valley.

We also looked at an area in the Imperial-Coachella Valleys to see what the virus activity was there. There had been some studies done there earlier by Dr. Telford Work from UCLA that showed both western and St. Louis viruses were active there. He was no longer doing these studies at a very intensive level.

What we found was that the Imperial-Coachella Valleys had a large population of *Culex tarsalis*. Both western and St. Louis viruses were active there, so we really had a hot spot. In the wintertime we could find virus; other people had not looked there enough in the wintertime to say whether it was there or not. Also, one of the hypotheses was that St. Louis virus, when it appeared in the metropolitan area of Los Angeles, had been imported by birds flying in from the Imperial-Coachella Valleys.

The difficulty with that hypothesis, which wasn't mine, was that we couldn't identify any bird that flew from the Imperial-Coachella Valleys to metropolitan Los Angeles in midsummer. There was certainly commerce and communication back and forth between the areas through the Banning-Palm Springs Gap. If I had to hypothesize, I'd say it is equally or more probable that produce trucks and other vehicles were more likely to be carrying infected mosquitoes north than it would be likely that viremic birds were flying it in.

Hughes: And *Culex tarsalis* does not fly that far?

Reeves: No, but there's no reason they can't be carried. *Culex tarsalis* has been shown to be introduced into Texas periodically from Mexico in freight cars that are carrying produce. There are certainly an awful lot of trucks that go out into fields in the Coachella-Imperial Valleys to collect produce, then close up their hatches and head north to unload in Los Angeles, or that go up highways 5 and 99 into central California to unload.

Hughes: There's no proof that they do this?

Reeves: No, but if you have to have an answer, I'd bet more on that sort of traffic than on bird movements.

The same thing has happened with tsetse flies in Africa, where they move sleeping sickness (African trypanosomiasis) from one region to another by traveling in vehicles. We know that commercial traffic is quite capable of moving mosquitoes. The introduction of *Aedes albopictus* from Japan into the United States is a beautiful example, and that was from the tire industry bringing used tires in that were infested with mosquitoes. They survived transportation in a sealed container in the hull of a

ship all the way from Japan to here. Then they were unloaded and turned loose.

Hughes: I've heard a similar theory for the transmission of yellow fever in water barrels on sailing ships.

Reeves: Yes, the mosquitoes breeding in water barrels on ships were free to bite the sailors and passengers. They could maintain the transmission cycle of yellow fever on sailing ships long enough to maintain virus activity all the way from Africa across to Latin America or the southern United States. This would require at least one cycle of amplification in a shipboard epidemic during the transit time. The same thing occurred when yellow fever was introduced into England in 1865. Dr. C. E. Gordon Smith and an associate wrote a paper on that.<sup>1</sup> It would have taken several cycles of transmission on the ship for the virus to have maintained itself.

Hughes: Was it recognized as yellow fever?

Reeves: Yes, but not the method of transmission. The ship came in and wasn't quarantined because it didn't report it had cases of yellow fever. It came into the harbor and moored, and then they had cases of yellow fever in residents who lived on shore in that immediate environment. The mosquitoes had left the boat, gone ashore, bitten the Englishmen, and given them yellow fever, and that really alerted them that something was going on. It was an interesting paper to see how the scene was reconstructed in 1986 using current knowledge of yellow fever.

Hughes: Tell me more about your research activities in southern California.

Reeves: We collaborated extensively with the mosquito abatement districts in the metropolitan area of Los Angeles for three years. We answered many questions about the flight range of the mosquitoes there, worked out details for sentinel chicken flock distribution, and compared different species of mosquitoes for their competence to vector St. Louis virus. We wanted to clarify further which species of mosquitoes were the important vectors and to characterize the virus strains that were isolated during the 1984 epidemic to see if they varied from the strains that we had collected in other areas, which they didn't. We've terminated these projects because we've answered their questions, and they're pretty much on their own as to what they want to do.

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<sup>1</sup>C. E. Gordon Smith, M. E. Gibson. Yellow Fever in South Wales, 1865. Medical History 30:322-340, 1986.



The Coachella-Imperial Valley area has continued to be a very interesting area to us, because we can continue to study the overwintering of viruses and can continue to do studies on mosquito biology which have not been done down there in enough detail to assist the mosquito control programs.

### Global Warming

Reeves: Another area we'll be getting into is global warming. If global warming does take place in California, the question is what's going to happen to virus infections. We find that having this hot spot down there--now, I use the term "hot spot" in a very broad way: it not only is a hot spot for virus but it's a hot spot in the summer as far as temperatures are concerned. It turns out that the year-round temperatures in the Imperial-Coachella Valleys average just about five degrees Celsius higher than they do in the Central Valley of California.

Hughes: What does that do to the virus?

Reeves: That's what we're studying now. Global warming may lead to a five degree Celsius increase in temperature in California. So here we have a natural experiment, where the Coachella-Imperial Valley area is five degrees higher than the Central Valley. Now we are getting data to compare on mosquito survival, virus activity, and the whole ball game in these two areas.

We are finding some interesting differences in the survival of mosquitoes at these different temperatures. It turns out that for every five degrees Celsius temperature rise there is about a 5 percent decrease in the mean daily survivorship of *Culex tarsalis*. So as temperatures go up, we find a really significant change in the length of time the mosquito will survive. In the Coachella Valley our life table studies show that they don't survive as long as they do in Kern County.

We can duplicate these different temperature regimes in the laboratory. We now are doing experiments in incubators, where we duplicate the temperatures that are occurring in nature in the Coachella Valley and in Kern County and seeing what happens to the virus in the mosquitoes at these different temperatures. We have put out what we call "data pods," which are little computer-chip recorders, in different mosquito habitats. We can put the probes from these data pods into the places where the mosquitoes sit in the daytime or where they come out at night. So we can actually

record the exact temperatures where the mosquitoes are, and we can actually put these data into the controls of an incubator, just like a clock, and it runs the temperatures up and down where the mosquitoes are kept, just as it happens in nature.

When we put virus into these mosquitoes and also study their survival, we get information on what ought to be happening in nature. It turns out that the data fit very well the natural survival and transmission rates. So this has led us into using these hot spots to gather data that not only will improve mosquito control but will allow us to anticipate what might happen to viruses with global warming.

Hughes: Was it your idea to get into global warming?

Reeves: Do you mean mine individually?

Hughes: Yes.

Reeves: Well, it's sort of peculiar how you get into these things. We weren't interested in global warming per se until about three years ago. The Environmental Protection Agency held a meeting in Washington at which they expressed concern over what was going to happen to diseases if global warming took place. So they brought a consultant group together, and I was invited to go. I said I wouldn't go unless Marilyn Milby could go with me, because she's my walking computer and thinker as far as knowing what data we have and being able to answer questions. She is a very competent individual in that regard. So the two of us went.

The only other persons there who were interested in mosquito-borne viruses were Bob Shope from Yale and Paul Reiter from CDC. They also were there as resource persons. Paul Reiter is from the Centers for Disease Control laboratory in Puerto Rico that is under the Fort Collins CDC laboratory. So there actually was a pretty good representation of people knowledgeable about mosquitoes and virus diseases. Other people dealt with dermatological problems and any other sorts of disease you wanted to name.

It was interesting that Dean [Joyce C.] Lashof's son, Dan Lashof, was one of the principal speakers for the EPA on what they anticipate will happen with temperature and ocean levels in the event of global warming. They gave us the big picture, but they really didn't have any detailed knowledge on what was going to happen in a small geographical area such as California. Their question was what would happen to the diseases you work with if global warming happens. Now, I asked them, "If you don't have a

crystal ball and God isn't your consultant, how do you answer a question like that when you've never thought about it before?"

They said, "What would you guess will happen?" Well, then your neurons start to work, and I said, "I wouldn't be surprised if western equine virus would disappear." That wasn't the answer they wanted. They wanted to know about diseases that were going to be a big problem, but they asked me, "Why do you think that?" I said, "At higher temperatures the mosquito cures itself of western infection, and I don't think that will be very good for the virus. It might disappear." So that interested them.

I said, "I think the St. Louis virus will do very well and tropical diseases might well be introduced. If you raise our temperatures, then we'll become more tropical. *Aedes aegypti* might adapt to California. You say there's going to be an increase in rainfall, and that will favor mosquitoes that are dependent upon water in artificial containers in the summertime. *Aedes albopictus* might also get to be common in California. There are various virus diseases"--and I listed some--"that might be introduced from the tropics and become endemic in the U.S." So I really just guessed off the top of my head. This interested them a great deal.

At the time of the meeting, it became obvious that EPA had no research money. I said, "If you want answers, you've got to buy them. It's that simple. You can't get research done without money." They said, "We're not a research organization." I said, "Well, then you'd better get NIH interested." Somebody has to decide this is a priority area and put money into it.

I heard nothing further from EPA, but we started getting much more interested in the effects of temperature on virus activity. As a matter of fact, we wrote global warming in as a major area for research in a research grant renewal application to NIH. I find some people say, "I don't care about it because I won't be here. This isn't going to happen until the middle of the next century, so I'm not interested in it." In response I have said, "I won't be here either, I don't think." [laughter] "If I am, I'm going to be the oldest man alive in the United States. However, it doesn't seem right to me for us to ignore global warming as a possibility, and it seems like a very challenging and interesting area for current research."

I didn't really hear anything more from EPA until this last year at the tropical medicine meetings in New Orleans. I suddenly got word that EPA and NIH were going to call a meeting the day after the tropical medicine meeting to take advantage of the national and international people who were there, to begin to



broaden their interest in global warming as an important factor as far as tropical diseases were concerned. But the two agencies were focused on the international aspects of tropical diseases and not on the United States.

That coincided with when I gave a paper<sup>1</sup> at the plenary session of the tropical medicine meetings on our work in California. Frankly, I was the only person who gave a paper at that session who wasn't talking in a solely theoretical vein about global warming and who actually had some data to present.

When we got into the special session organized by EPA and NIH, there were about twenty of us in attendance, and we represented a wide range of diseases--malaria, leishmaniasis, schistomaniasis, any iasis that you wanted to know about. Again, it turned out that the agencies wanted to stress the important international diseases, such as malaria, schistomaniasis and so on, and wanted to find databanks that could be used to develop models that would be directly applicable to the question of what was going to happen when global warming takes place internationally. It was believed that would impress the Congress. They just happened to be thinking about it that way. However, it turned out in the discussion that for most diseases there wasn't a lot of data on the impact of different temperatures on the life tables of vectors or on development of the parasites in their vectors. We were really the only people who had been working intensively in that area.

Hughes: Do you have any idea exactly where in this complex cycle temperature is having its effect?

Reeves: Increased temperature is shortening the life table of the vector, and lower temperature is increasing the life table until the temperature gets very low. This, then, is a major factor that will control whether the mosquitoes are going to live long enough to incubate the virus or any other pathogen to the point they can transmit it. If they don't complete their incubation of the virus, nothing's going to happen except the virus will disappear. The virus can't be maintained.

Hughes: Is it as simple as, above a certain temperature the mosquito can't survive?

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<sup>1</sup>W. C. Reeves. Climatological changes and the competence, capacity, and distribution of mosquitoes as vectors of arboviruses. Presented at Plenary Session Amer. Society Tropical Medicine and Hygiene. New Orleans, Nov. 5, 1990.

Reeves: Not a certain temperature as a threshold. As you increase or decrease the temperature, say by five degrees, you get a 5 percent change in the survival of mosquitoes in a population. It's a fairly simple, straightforward thing. You can draw curves that represent it, and our data in both the field and the lab follow these curves pretty closely. So you can estimate the probable change in survival with any increase in temperature, and then you can insert the incubation periods of the viruses in the vector at those temperatures and see how the two variables are going to work against each other.

Finally, with western virus as a model, higher temperatures allow the mosquitoes to cure themselves by some unknown mechanism. That's very critical, as the mosquito may live but may cure itself, and the virus will disappear or decrease to the point it no longer is a good vector. That's a very critical thing from the virus's viewpoint, not from the mosquito's and not from our health viewpoint. Well, it could be from our health viewpoint, but on the favorable side.

Those are some of the factors that we've identified at this stage. Now, there are a lot of other things that we could get into. For instance, Jim Hardy is almost convinced that a heat-stress protein is developed by mosquitoes at higher temperatures and may actually function as an antiviral type of a substance. We have no direct data on that, but it could open a whole new area for research.

Hughes: Do you have any more comments on the NIH-EPA meeting in New Orleans?

Reeves: Yes. At the end of that meeting they were still trying to decide which diseases to pick for the models they wanted to convince the Congress and other bodies that funding for global warming should be made a priority area for research. They finally said, "We're supposed to be focusing our concern on the important diseases internationally--malaria, schisto, etc. These are the diseases that will sell this sort of a program, as they represent an important health problem in many regions."

I said, "Okay, fine. I really don't believe you should use St. Louis or western encephalitis as your model if that is your interest. You invited me here. I didn't invite myself. If you want a model of what can be done in certain areas, the research I presented at the earlier meeting is the sort of thing that can be done. But I wouldn't try to sell the program on the basis of St. Louis encephalitis. Even the outbreak of over two hundred cases in Florida this year and the shutting down of Disneyland and



national parks isn't going to be enough to sell that. I think you're wise not to try to use St. Louis encephalitis." Fine.

At this time a representative for EPA said, "I still plan to turn a group loose modeling what you've done and to use your data." I listened to this with interest. Finally the chairman said, "What do you have to say about that, Bill?" I said, "Well, he hasn't bought the data from me yet." This guy said, "What do you mean?" I said, "It's my data and represents work of my colleagues. It's not just mine. I don't have any reason to give you our data that hasn't been published yet. In fact, I have a graduate student who is working on modeling much of our data. Why should I give it to you?" "Oh," he said, "I didn't understand that." The chairman looked at him and said, "Do you understand now?" He said, "Well, we're not a research agency. We don't have money for research." I said, "Fine. I didn't say that you couldn't have the data. I just said I haven't sold it to you yet."

I talked to my colleagues, and we agreed we would give them the basic data that I'd given in the paper. It's not going to be published in the near future, but we'd give the information to them. So I sent a copy back--not to the young fellow that I put in the corner, but to another person. He called me in a couple weeks and said, "You know, Bill, we had another meeting and we have decided that we'd better extend our interests to domestic as well as international diseases, and we need your data on western equine and St. Louis encephalitis. You sent me a copy of that paper, didn't you?" I said, "Yes. I sent you the laser printout of the figures and the text, and I hope you haven't lost it." I haven't heard anything further from EPA or NIH.

Meanwhile, some funding has been given to the Davis campus from several sources, and they're now asking for applications for funding from people in the university for studies on various aspects of global warming. The basic information they sent out about that money said nothing about public health. However, we sent in an application for \$100,000 to extend our research on global warming in the Coachella-Imperial valleys, Bakersfield, and Berkeley. Dr. Eldridge from Davis also asked for money for research on the effects of global warming on the mosquitoes and the viruses in the salt marsh environment. As of this week we were informed that neither of these proposals would be funded, and we are waiting for further explanation of why.

We'll wait and see what happens, but I think they are much more concerned with the economic impact of global warming on agriculture, forestry, and water resources in California than they are on public health issues. Marilyn's gone to several meetings



of the Davis group, and they sit there for days considering all aspects of global warming except health. She's the only person in the audience who speaks up and says, "What about health?" The attendants are mostly economists, agriculturists, environmentalists, and so on, and they just don't know or care that much about health at this time. But we're still doing our best to create an interest in health.

### Research on Snow and Salt Marsh Mosquitoes

Hughes: Let's talk about snow mosquitoes and salt marsh mosquitoes.

Reeves: Dr. Bruce Eldridge came to the University of California staff in May 1986. He applied to be the coordinator for mosquito research in the University of California. This program is run out of the Division of Agriculture and Natural Resources at Davis and is responsible to the president's office. It distributes the money that the state legislature has allocated to the University of California, which is something close to \$500,000 a year to fund research in California on mosquito control, mosquito vectors of diseases, and related subjects.

Bruce has a very broad background in medical entomology and virology. He was the head of the department of entomology for Walter Reed Army Institute of Research and retired as a colonel and then went up to Corvallis as chairman of the entomology department at Oregon State University.

One of the interests that he took with him was a continued interest in mosquitoes and viruses in the California complex. We originally isolated California encephalitis virus from *Aedes* mosquitoes in Kern County. While Bruce was in Oregon he collected mosquitoes and started studying their relationships to viruses and also studied blood samples, primarily from deer that he got from the Oregon State Fish and Game Agency. He worked with Charles Calisher at the Centers for Disease Control at Fort Collins, Colorado.

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Reeves: The position in California became vacant when Dr. Russell Fontaine retired, and Bruce applied. We had a number of very excellent candidates for the job. He was selected, and I was very pleased that he was in the sense that his interests and ours were very similar and I'd known him for a number of years.

After arrival at Davis, he came down one day to talk to Jim Hardy and me about collaborating with him to extend the studies he had started on the mountain *Aedes* and related viruses in the western United States. These mosquitoes are dependent upon snowmelt--melting water from snow--to flood low spots in the high mountain environment. They lay their eggs there each year, and the eggs sit there in the ground all through the winter with snow and ice covering them. As a matter of fact, they have to be held at low temperatures for these eggs to mature. When they're flooded the following year with the snowmelt, which is very cold, the eggs hatch. So there are eggs just sitting there ready for the trigger of melted snow water to flood them. They come out as a single cohort; that is, a single brood of mosquitoes comes out each year when the conditions are right.

So they're very peculiar mosquitoes, and they get involved as vectors of this particular group of viruses. The only way this mosquito survives the winter is through the eggs. There are no adult mosquitoes there in the winter, and no blood feeding going on. The virus when in mammals, like deer or rodents, produces a very transient viremia only lasting for three or four to five days. The mammals then become immune, so they can no longer be a source of virus infection. So the virus is really dependent upon the eggs; that is, the females transmit the virus transovarially to their progeny.

We started the project with Bruce and very quickly found that we could isolate Jamestown Canyon virus from mosquitoes collected in the Sierras near Tahoe. We confirmed that the virus was transmitted through the egg, as the initial virus isolations we made were from mosquitoes that we hatched from larvae and pupae brought from the field into the laboratory, so they'd never taken a blood meal. We tested the adults that hatched out, and they were already infected with virus.

We got a National Institutes of Health research grant to study in detail the relationships of these viruses to the complex of mosquitoes that are in that environment and also, because Bruce was very interested in it, approval to study the evolution of mosquitoes in this environment. The mosquitoes in the high mountains are also related to the salt marsh mosquitoes along the coast, and it is hypothesized that there's an evolutionary relationship between them, even though the populations have been separated from each other probably for many, many centuries.

Our task was to determine to what extent viruses were in these mosquitoes, to what extent the viruses were related, and which of the species the viruses would appear in were related to each other. Bruce brought a postdoctoral student in, Gregory C.



Lanzaro, who was very sophisticated in the latest methods of differentiating mosquito species. He could take a sample of the individual mosquitoes and by electrophoresis separate the enzymes from each mosquito and show to what degree the enzymes indicated there was a relationship. I won't go into more detail on that technology.

To make a long story short, it turned out that some of the mosquitoes we thought were single species in the mountains and coastal areas were indeed a complex of several species that had not been separated taxonomically before. Also, it turned out that the viruses were somewhat specific to the mosquitoes infected with them. We found the Jamestown Canyon virus, which is very closely related to California virus, in the mountain environment; as a matter of fact, it's the only virus we have found there. At Morro Bay we isolated another virus out of a salt marsh mosquito, which turned out to be very closely related to the classical California encephalitis virus that we had originally isolated in 1943 from *Aedes melanimon* collected in Kern County. So now we have different viruses in both populations, the high mountain mosquitoes and the coastal mosquitoes, and this is the first evidence in California that there is any virus activity in mosquitoes in either of these habitats. We are now finding at least two additional viruses in mosquitoes at Morro Bay.

Now, these findings were of particular interest to the people that run mosquito abatement districts in the coastal areas because they have long been controlling salt marsh mosquitoes solely because they're such a severe pest. But they've never had any viruses associated with them before, and now they can use the virus information as another reason to control the mosquitoes.

At the beginning of these studies, Dr. Roy [Grant L.] Campbell was a doctoral student in epidemiology who was looking for a thesis problem. Roy said, "Can I work on this project?" Roy is a physician, and he became quite well trained in virology and entomology as well as in epidemiology. He did an amazing job in a short period of time. He took on the problem of seeing if there was any evidence of infection with this virus in animals.

He found that deer in the high mountains had antibodies to the Jamestown Canyon virus. He found that he could measure virus activity by detecting antibodies in cattle that were taken from low elevations into the high mountain areas for the summertime. He worked with Dr. Benny Osburn and his research group at the veterinary school at Davis to get the cattle samples and with David A. Jessup of the State of California Department of Fish and Game to get deer samples. Roy also collected blood samples from forestry students that were in a forestry summer training school



up in northern areas of California and showed that some had antibodies but none developed antibodies during the one summer period of study. He collected serum samples from foresters and other people who worked in the high mountain areas and found out that an increasing proportion had antibodies, depending on how long they'd worked in this environment. So clearly, people were being infected. His efforts to find evidence of a clinical disease associated with the virus infection did not succeed, but he made a serious effort to do that. But that doesn't mean there is none, and at least it opened that door for further study.

We brought mosquitoes collected in the various environments back to the laboratory, and Laura Kramer did vector competence work with the various species; we did the whole game. I spent most of one spring and summer up at Tahoe doing mark-release-recapture studies on almost six thousand marked mosquitoes. This had never been done in that environment before, and we showed that they had extremely long survival. The mosquitoes would hatch out, and some of them were still there six weeks later--with less than a 5 percent mortality per day. They just stayed there, chewed on you, and laid more eggs. It was an interesting summer. We've begun to study details of these mosquitoes that have not been studied before, finding out that there are more species than we thought there were and evaluating which ones can be vectors for these viruses. We think that we'll eventually find out to what extent they can transmit viruses that might be disease problems to people.

Roy finished his thesis<sup>1</sup> and got a Ph.D. in epidemiology. He is now at the Centers for Disease Control research facility in Fort Collins, where he's the epidemiologist on their Lyme disease studies and doing very well. Lyme disease is a tick-borne spirochete disease. Another doctoral student, Chuck Fulhorst, is now working on the salt marsh project. All last year, before he came, we were testing salt marsh mosquitoes. We didn't find any virus activity, which was very disturbing, as we had made five isolations the year before from Morro Bay. This year Chuck has collected more mosquitoes than we did all last year, and we already have five additional isolations of virus from the salt marsh habitat at Morro Bay. So at least we have one "hot spot" where we can concentrate our studies. This weekend he's going to Los Angeles County to make collections all up and down the coast at different salt marshes. He will also contact veterinarians to collect animal bloods. I made four trips in the last week to

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<sup>1</sup>G. L. Campbell. Epidemiology of California and Bunyamwera Serogroup Viruses in Mountainous and Other Areas of California. 1990. Ph.D. thesis, University of California, Berkeley.

collect mosquitoes for the laboratory to do vector competence work on the species of mosquitoes from the salt marshes. In these studies we'll learn more about the mosquito's abilities to transmit viruses. Dr. Laura Kramer is doing that work.

Hughes: Is this project related to the discovery of emerging diseases?

Reeves: We haven't found any newly emerging disease, but we have found an emerging possible problem of viruses associated with new groups of mosquitoes in a completely different ecological environment which have never studied before. We find it rather exciting and interesting to be working on such new problems. The students in classes frequently say, "There's nothing new. You guys have done it all." That's not true, and I will also confess that it's much nicer to do field work in the Sierras in the springtime and summer than it is in Kern County in July where it is very hot.

Hughes: The fishing is better.

Reeves: I haven't done that much fishing up there because there are too many people and not enough fish. I don't like to fish in an area where there's somebody standing on every rock. I've caught a few small fish up in the Sierras. We were usually working too hard--sixteen to eighteen hours a day. You collect mosquitoes as hard as you can, and then you have to get them back to Davis alive. They're very delicate, and if you don't get them back alive, you have wasted your time and don't have anything to work with. Or you have to get them back to Berkeley for vector competence work. You work like crazy day and night in the field for a tough three or four days and come back and recuperate for a couple of days. Some days I wonder why a guy seventy-four years of age is doing these dumb things [laughter], but it's a lot of fun. I can work at 8,000 and 9,000-foot elevations, and the altitude doesn't bother me.

We have a five-year grant from the National Institutes of Health for research on snow and salt marsh mosquitoes. Our concern has been this drought; it hasn't helped us a bit, because there are some areas that should get flooded from snowmelt, and there's been no snow. The NIH isn't giving us money to study the effect of the drought on snow mosquitoes. Until this last storm, we wondered if we were going to have any mosquitoes to work on in the Sierra and salt marshes. The salt marshes were dry; they hadn't flooded yet. We now have enough snow in the Sierras that we'll have mosquitoes, and the salt marshes are flooded by recent rains. So we have an agenda laid out of things to do and questions to answer.



### Emerging Viruses

Hughes: Dr. Reeves, I know you've just come back from a meeting in Washington about emerging diseases. Do you want to say something about that?

Reeves: There is a study going on by the Institute of Medicine of the National Academy of Sciences on emerging microbes that might be of future public health concern in the United States and other regions of the world. This committee [on microbial threats to health] just had its first meeting. The basic objective is to develop policy statements on what the U.S. government's concerns and actions should be. It's too early to say more.

We have been interested in emerging viruses historically in California because in fact western and St. Louis encephalitis viruses emerged as new diseases in California and in the western United States in the late 1920s and the early 1930s, and we've had the privilege of studying them all this time. We know there are many additional viruses that occur in wildlife and that have mosquito vectors in California, and all of these have the potential of getting into people and causing diseases and then seeming to be newly emerging diseases. We now know the viruses are here. We say they are looking for a disease to be associated with. This is true all over the world.

In the arboviruses alone we now have over five hundred viruses, yet only a little over a hundred have ever been associated with a human disease. But we know they're in nature, they're out there, they're being transmitted between mosquitoes or other arthropods and animal hosts. We know that a lot of these viruses are occasionally infecting people because we can find people with antibodies. It may well be that we have not been looking for the right disease to associate them with, or the virus and the disease haven't come together yet in our knowledge, even though they're already there.

Hughes: Are you safe in assuming that an infection leads to disease?

Reeves: No, not at all. As a matter of fact, we know that with the viruses we know a lot about, like western or St. Louis encephalitis, there may be five hundred or even a thousand people who get an inapparent infection with no clinical disease for every clinical case of encephalitis that is identified where virus has entered the brain. The same thing can be true for other viruses; even for yellow fever, people can get infected without getting an



illness. The virus has to get into an organ where it can do enough damage to produce a disease.

For instance, in the central nervous system diseases--polio, encephalitis--it is common to say that there's a blood-brain barrier that prevents entry into the central nervous system. In other words, there's a virus in the bloodstream, but enough doesn't get across this barrier, whatever it may be, and into the brain to cause damage. As a matter of fact, most of these infections in their natural animal hosts do not lead to a disease. Sometimes it's when viruses get into an abnormal host that they cause the disease, not that the host is necessarily going to perpetuate the infection and be a source for vector infection.

Our concern is whether there are a lot of viruses which haven't had an effective contact with man so as to give the virus a chance to express itself in people or to adapt to people enough that it is then effectively retransmitted. One of the primary hypotheses with AIDS is that it was not originally a virus infection of people. It was originally an infection of other primates, and it just got into man and then was adaptable to being transmitted by blood or sexual contact. I don't know anyone who thinks that AIDS is a new virus that just emerged out of nowhere. It probably is a virus or viruses that have been around for a long time, and suddenly increased movement of infected people, extension of social relationships, and sexual activities led to increased and effective transmission.

When we started working on encephalitis, we knew in a short time that we had at least two viruses that were causing disease--western equine and St. Louis encephalitis. In 1943, we discovered that there was another virus in our mosquitoes, and that was California encephalitis virus. We named it that because it was isolated in Kern County, California. As I told you, we tried very hard to get evidence that it was causing an important disease in man, because at least half of the cases of encephalitis that come into the hospital and into the laboratory for diagnosis can never be diagnosed. So there are a lot of people out there with an infection of the brain of some type that we don't have a cause for. We looked at blood samples from about a thousand of these cases in the laboratory, and we finally identified three cases of California encephalitis out of those thousand people who were sick. That's not enough to be a big deal. But we also found that if we collected blood from normal people in Kern County, a lot of them had antibodies to this virus, so they were being infected. However, it wasn't important enough as a disease agent for us to spend our time and energy on it, so we put that virus in the Revco freezer to preserve it and forgot about it.

Subsequently, a new disease called LaCrosse encephalitis showed up in Wisconsin and adjacent states. Actually, that virus turned out to be related to California encephalitis virus, so that created more of an interest in our virus, but it still wasn't of that much interest to us. Our snow mosquito project again showed that another virus related to California encephalitis, the Jamestown Canyon virus, is here, and we also isolated it in Kern County from mosquitoes collected there. As I said earlier, we also have California encephalitis virus in the salt marsh mosquitoes. But again, so far we haven't been able to associate it with disease. We have some twenty additional viruses that we know are in California, most of which have mosquitoes or other blood-sucking insects as their vectors, and they're a common infection in wildlife. A few of these we have been able to associate with disease, but only with a few cases.

As an example, we had a case of aseptic meningitis in a young boy down in Kern County some years ago, and we screened through a whole bunch of viruses that we had isolated to see if by chance it might be caused by one of them. To our amazement, it turned out that this boy had been infected with Modoc virus. Modoc virus is not new to you, because Dr. Johnson undoubtedly talked a great deal about Modoc virus.<sup>1</sup> This is the only case of Modoc virus infection in man that's ever been recorded. This boy had meningitis, a very serious disease, was hospitalized, and that's the only virus to which he showed a rise in antibodies, so that's probably the virus that infected him.

I did an epidemiological history on this boy and his family and asked him what he'd been doing several weeks before he became ill. Well, the only thing we found that was different was that the family had gone up to spend spring vacation in a cabin they had in the Sierras above Porterville in Tulare County. I said, "What did you do when you were there?" He and his two brothers had spent considerable time catching and playing with little wild white-footed mice, and the mice had peed on them. They were very impressed with this, as only boys can be; you know, this was worth talking about. It was very interesting, because this is the same species of mouse that Modoc virus was isolated from by Dr. Harald Johnson. He isolated Modoc virus from the mammary tissues of the same species of mouse. This boy had a beautiful history; he'd been exposed to these mice at the right time before he became ill. Now, we went to where he'd been, and we collected mice, but we couldn't get virus out of them.

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<sup>1</sup>See the oral history with Harald Norlin Johnson.



We or other people now have some twenty viruses that we have isolated from various types of vectors or animals in California. We're constantly looking to see whether they are associated with disease. The State Health Department gets clinical specimens from patients, and they run through the expected causes, like western and St. Louis encephalitis, polio, and whatever. Then they're through with those sera; they've answered the physician's question, and they aren't the diseases the physician suspects. But we don't let them throw those sera away; we put them into a bank, and we have several thousand paired sera from such cases. We can now go back and see if any of the other viruses or new viruses caused the infection.

Some of these viruses you might say are very likely candidates. Dr. Harald Johnson isolated Rio Bravo bat virus from the salivary glands of Mexican free-tail bats in Kern County. This virus has caused illness in humans. He isolated Kern Canyon virus from the salivary glands of bats. This virus never has been associated with a disease, but there it is in bats, and a lot of bat contact with people takes place. Rabies can be transmitted to humans from the same species of bat, and they potentially can shed the new viruses in their feces, urine, or salivary secretions. Inevitably, man is going to come into contact with these viruses.

We have viruses that are very common in *Culicoides variipennis*, which is a very common little gnat in the Central Valley of California. This gnat is the primary vector of two diseases--bluetongue in sheep and cattle and epizootic hemorrhagic fever in deer. As I mentioned, we started looking at *Culicoides* in Kern County to see if they were carrying western or St. Louis virus. We never got those viruses out of them, but we got three new viruses. We gave them these unique names that I think I mentioned before--Buttonwillow, Main Drain, and Lokern virus. The state virus laboratory has now isolated Main Drain virus from the brain of a horse with encephalitis. Again, these viruses all have the potential to come in contact with people or with domestic animals and to evolve as the cause of a disease of considerable importance. So far we have no evidence of that, but we know they're here.

I could name a very extensive list of viruses that we haven't yet associated with a disease, and they occur in a wide array of habitats. These habitats are usually not in developed areas. Mono Lake virus came from the east side of the Sierras. That isn't exactly a place where a lot of people go for vacation, but increasingly they are. Some of these viruses are from high mountain areas or deserts, and these are increasingly used as recreational areas.



Early on Dr. Emmons showed that some of the California group viruses were active in deer in the Yosemite Valley. As we do surveys on deer or cattle or whatever in Sierra habitats, we find that they're getting infected. When we get human samples, we find people are being infected, but we don't know whether a disease is associated with the infection.

The difficulty is, if somebody goes to Tahoe or Yosemite on a vacation they stay there for a week or two, and then they go home. Two weeks later they get sick, and they go to see their doctor. He may get a history, and they'll say, "I was up in Yosemite." That doesn't mean anything to him. He doesn't know of any diseases you're going to get from Yosemite; it's supposed to be a nice, healthy place to go for a vacation. He doesn't have any idea what the disease is, and he treats it symptomatically; and the samples never come to us.

We found that Modoc case in Kern County because I had some medical students who were there that year taking blood from any febrile illness or CNS [central nervous system] case that came in to Kern General Hospital. The sera from that case came out of bloods they collected. Our problem is that we have all of these viruses here, and we have human populations increasingly being exposed to agents as they go into nature for recreational purposes. We also have constant movements of populations taking place in California. Go to any of the foothill areas of the Sierras or marginal areas of the Central Valley, and there are big new cities or housing developments out there. People are moving into the environments where these viruses are, and that's going to increase their contact. We've just had another probable case of plague reported in a resident of California. This again will represent a case where man has intruded upon an environment where rodents are infected with plague. He got infected from fleas from the rodent, or his pet cat or dog picked up a dead animal and got infected from this contact and passed it on to its owner. I don't know the details of this case yet, because it hasn't been worked up. There have been plenty of such cases earlier to set the epidemiological pattern.

I can extend this list to include Colorado tick fever and Rocky Mountain spotted fever. These diseases are contracted when people invade northern California areas where they are exposed to *Dermacentor* ticks. Lyme disease, a tick-borne bacterial infection, is another example. This undoubtedly isn't a new disease in California. There is serological evidence that it was here in the forties, but it was recently recognized as a relatively common disease.

We have no idea to what degree some change has to take place in an agent for it to be transmitted directly from person to person, like AIDS or, occasionally, the hemorrhagic fevers. There are still new hemorrhagic fevers emerging. The group at Yale just isolated a new hemorrhagic fever virus from a patient in Venezuela. They have no idea yet exactly what it is, but it has caused a number of human cases and some deaths. We know the virus group it is in, as it is related to the other South American hemorrhagic fevers, but it looks like a previously unknown virus.

Hughes: Why were you appointed to the Committee on Microbial Threats to Health of the National Academy of Sciences?

Reeves: I think it was in recognition of our group in California knowing a lot about such agents because of our work, plus the fact that they needed to have the arbovirus area represented. So they put Dr. Shope and me on the committee. He represents the WHO International Reference Center for arboviruses and has worked extensively in the tropics. I've worked extensively in the domestic scene here on arboviruses, and the committee is directed to deal in large part with the possibility that some new diseases will emerge or be recognized in the United States. I think I was named to give a balance in viewpoints. There are many other people on this committee who represent molecular biology, vaccination, influenza, parasitic diseases, or any other group of infectious diseases that you want to name. So they're going to have as broad a representation on the committee and its four subcommittees as they can, within the budget they have.

#### The Monograph, 1990##

Hughes: Dr. Reeves, your second monograph on research on encephalitis very recently came out.<sup>1</sup> Could you give me a little background?

Reeves: When you've worked for all the years that we have on a specific research problem, you accumulate an enormous databank. You come to the realization that it would be of some interest and value to put the various studies together in one publication. The first time we did this was when Dr. Hammon and I published a monograph

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<sup>1</sup>W. C. Reeves in collaboration with S. M. Asman, J. L. Hardy, M. M. Milby, W. K. Reisen. Epidemiology and Control of Mosquito-borne Arboviruses in California, 1943-1987. Sacramento, CA: California Mosquito and Vector Control Association, 1990.



that summarized the first ten years of our work.<sup>1</sup> That summarized the studies up through 1952. It covered the ten-year period that we worked on together very closely, even though he'd left for the last few years of that period and gone to Pittsburgh in 1949.

Subsequent to 1952 several hundred additional papers had come out on the research, and in the early 1980s representatives from mosquito control districts and state and local health department agencies said, "Why don't you write another summary of what has been done and describe the relationships of these various studies?" I wasn't overly enthusiastic to do this, because I was looking at a mountain of data, and I was going to have to reduce it to a molehill. But my colleagues and I agreed we would do it.

We tried to get funding from NIH for this project, and they said, "That's a California problem; let California finance it," which I thought was very short-sighted, because we've never done anything solely because it was a California problem. We thought our studies had broad applications inscience and to a large geographical area. As a matter of fact, initially we couldn't find anybody who wanted to finance the project; they just wanted it to be done.

We got some indirect support from state research money and some support from the California Mosquito and Vector Control Association to get graphs and other figures put together. Then the CMVCA said they wanted to publish the monograph in their publication series. It was something that they had implemented. They wrote letters to the dean [of the School of Public Health, Joyce C. Lashof, asking her to make me do this. She sort of laughed about it, because the dean knew that she couldn't make me do anything [laughs]. Letters came in from the president of the CMCVA, Dr. Frank W. Pelsue, and the president of the American Mosquito Control Association, Gilbert L. Challet.

I knew I wasn't going to do this by myself, but I also knew that I was extremely fortunate in the close associates that I had. Four associates, James L. Hardy, Marilyn M. Milby, William K. Reisen, and Monica Asman, indicated very quickly that they would be willing to collaborate and indeed to take primary responsibility for many of the chapters. So we evolved a plan to develop ten chapters that would divide up the work.

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<sup>1</sup>W. C. Reeves and W. McD. Hammon. Epidemiology of the Arthropod-borne Viral Encephalitides in Kern County, California 1943-1952. University of California Publications in Public Health, vol. 4. Berkeley: University of California Press, 1962.



Bill Reisen would do mosquitoes and mosquito biology, and I would collaborate with him. Sister Monica Asman would take the primary responsibility for mosquito genetics, because indeed that was her area, and Marilyn Milby and I would collaborate on some of the field aspects. I agreed to do the human disease chapter and some of the summary chapters that dealt with virus overwintering, surveillance systems, and the experimental control studies. Marilyn Milby, because she'd handled all the databanks, would do animal hosts, and I collaborated with her on that. Jim Hardy and I would summarize the extensive research on vector and host competence.

We developed a very nice plan where Marilyn Milby would take a lot of responsibility for integrating chapters and putting them into her computer system. She would be responsible for making multiple drafts, which would be a big workload. I provided the various people who were doing first editions of various chapters with outlines, basic reprint selections, and tearouts from our annual reports. To my amazement, Sister Monica was the first person to finish her chapter. She was retiring and was anxious to get back to the convent and religious commitments, so it was urgent to get her chapter done. She did it very rapidly, and it is a very good chapter. It was sort of fun to have her chapter to wave at other people, including myself.

We plodded along, and as we could find time we managed to get all the chapters into drafts. It took us five years. One of the nice things about this book is that it also brings together basic data that was put in the first monograph. This had a lot of historical findings that led us into other things that we did. The later data answered many old questions and applied this knowledge to today's world. I think we've been able to make some sense of the evolution of the overall project and the changes and directions that took place. It is a resource for anyone who wants to get at the overall picture--the amount of disease that's taken place over the years, the impact of various approaches to control. I hope it really brings the whole scenario together in one place.

The research is not going to be done again. It's been extremely expensive. I don't even have an estimate of the cost. Our total research budget over the years was several million dollars. That's a lot of money to go out and shake the apple tree, as Dr. Meyer used to say, and get enough apples down to add up to the necessary support. We used support from the National Institutes of Health, the army, the state, State Fish and Game--any place that we could find resources. In today's world, it's hard to maintain that level of support. I don't think we'll be able to continue it at anything like that level in the future, with the economy of science being what it is today.

It was a real challenge to get it together. It was a real challenge to pound it down, and maybe we didn't pound it down as much as we should have; it's still five hundred pages long. There's a lot of data, and we present a lot of the thinking process, and there are a lot of the interpretations made, plus it makes the data available for other people. In addition, it gives people an idea of what basic data we do have on the computer. As far as we're concerned, those data are very available to others. I guess that you might call this a final curtain act. But at the same time, it doesn't seem that way, as we are involved in a whole bunch of new projects.

Finally, Marilyn had to clean up multiple drafts and set the final copy ready for reproduction, which was a major task. John Combs, the executive secretary for the California Mosquito and Vector Control Association, was a constant supporter and stimulant, and that organization is the publisher. He is also the manager of the Delta Mosquito and Vector Control District. This means there will be minimal problems with copyrights. Another of the values of having them be the publisher was that the cost of the monograph is kept low. I was concerned that most of the publications in science that come out now are so expensive that no student, few professionals, and few libraries can afford them. A series of four volumes will come out in some field like arbovirology at a hundred-some dollars a volume, and who can afford to put out \$500 to have four books sitting on their shelves?

We gave the manuscript to the California Mosquito and Vector Control Association copy-ready, off the computer, so they had no modifications to make. They put it out for less than \$30 for the paperback copy and less than \$50 for the hardbound copy, and that's a bargain in today's publications. And it's nicely put together. Basically, the only problem in having a group like the CMVCA put it out is that it will not be backed up by a commercial advertising setup. The CMVCA is a novice in this business but will use book reviews and sell it at meetings like the New Orleans meeting of the American Mosquito Control Association that I'm going to next week. They'll be selling copies there, and I'll be there if anybody wants my autograph on a copy. That's really the principal reason I'm going to the meeting. They've got a thousand copies sitting in Sacramento and figure they have to sell between four and five hundred copies to break even. It may sell like hotcakes. I don't know. My children, to whom I gave copies, are sort of hesitant whenever I phone them. They say, "No, we haven't finished reading it yet." [laughter] None of my grandchildren have admitted they've even looked at it, and my wife certainly

hasn't even looked at it yet. You haven't looked at the copy I gave you, either.

Hughes: I have so. [laughter]

Reeves: Marilyn Milby was really the slave driver on this project. She was constantly goading people. This was the biggest project she was ever involved in. She was just the person to do this sort of thing. It isn't that often that you see somebody in her position being an author on a large portion of the chapters. But she's an unusual person. I am very fortunate to have dedicated colleagues and staff.

The monograph covers everything we've done on encephalitis in California in almost fifty years. Well, it covers more than fifty years; it goes back to K. F. Meyer, 1930.

Hughes: It's a wonderful history.







William Claude Reeves, Alice Brant Reeves, and Billy.  
Circa 1919.







Bill Reeves and Mary Jane Moulton, the future Mrs. Reeves. Riverside Junior College students, 1936.





William C. Reeves and William Hammon collecting mosquitoes in the Yakima Valley, Washington, 1941.







William C. Reeves and Barney Brookman with original light trap to collect live mosquitoes, 1942.

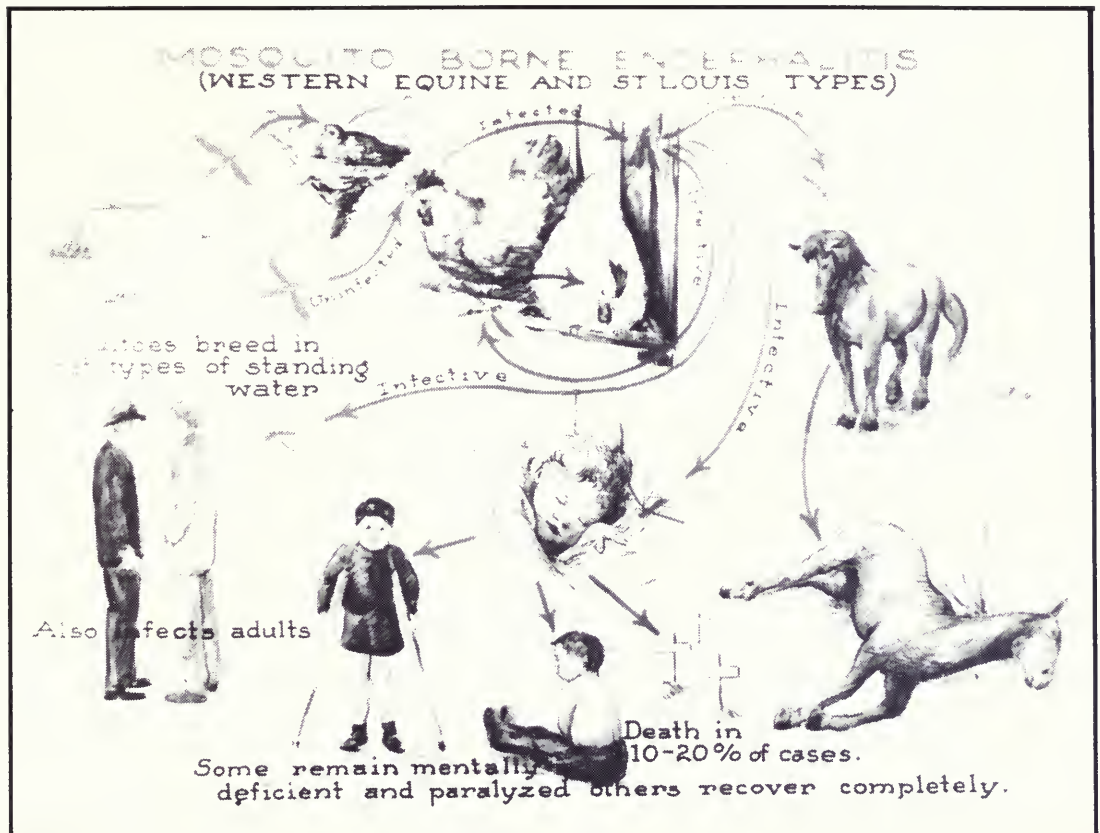






The field team, Yakima Valley, Washington, 1948. Left to right: Dr. H. Elliott McClure, Dr. L. B. Fastier, Mrs. Doetschman, Dr. Richard O. Hayes, William Allen Longshore, Jr., Dr. William McDowell Hammon, Dr. W. H. Doetschman, Dr. William C. Reeves, Dr. Bernard Brookman, Dr. A. S. Lazarus.





Life cycle of mosquito-borne encephalitis.







William, Mary Jane, Robert, William, and Terrance Reeves, 1949.







William C. Reeves and Malcolm Simpson collecting mosquitoes in the Murray Valley, Australia, 1952.





Anitol Smorodinstev (Leningrad, Russia), William C. Reeves, and Carl Eklund (Hamilton, Montana). Photograph taken at the International Congress of Tropical Medicine and Malariology, Lisbon, Portugal, 1958.







Celebrating the publication of the 1990 monograph. Left to right: John Combs, William C. Reeves, William Reisen, Marilyn Milby, James Hardy, and S. Monica Asman.





## VI THE DEVELOPMENT OF ARBOVIROLOGY

[Interview 8: April 3, 1991]##

### Arbovirology in World War II

Hughes: Dr. Reeves, there was a surge of interest in arboviruses after World War II. Could you talk about what happened during the war to promote that interest?

Reeves: During the war we had a tremendous number of troops who were sent into the Pacific, African, and European theaters. As a very specific example, in this period in 1943 there was a major epidemic of dengue fever in Hawaii that incapacitated a large number of troops that were being staged there for shipping to various parts of the Pacific as well as the civilians who operated this critical port city. That dengue outbreak was so extensive that it threatened to close the port of Honolulu, which was critical for the navy and for transshipment of troops, supplies, and everything else into the Pacific.

We didn't know too much about dengue fever at that time, there was no vaccine available, and we had no magic way to control the very common mosquito vector. It was *Aedes aegypti*, the yellow fever mosquito, but it was carrying dengue in this case. That epidemic actually was the source for one of the first of the four prototype dengue strains that are now known. Dengue was the only mosquito-borne disease present in Hawaii at that time.

As the troops moved into Guadalcanal, New Guinea, and all through the Pacific, they began to run into dengue again. The second prototype strain of dengue that was isolated came out of New Guinea in the early 1940s. It's called the New Guinea strain. So we had troops who were being incapacitated, though they weren't dying from it. The hemorrhagic fever and shock syndromes that are

now associated with dengue hadn't been discovered yet; that was done some years after the war by Hammon in the Philippines. But basically, dengue was a major incapacitating disease.

When the troops hit the Pacific islands in the far Pacific, they also immediately ran into malaria. At that time we didn't have any of the curative drugs that we have now for malaria, so we were dependent upon using quinine to treat acute cases. That helped them some, but it didn't cure them. Our methods for mosquito control were very primitive then; we had none of the current insecticides. It wasn't practical to have troops sleeping under bed nets, and we had no good repellent to use, so malaria was really a major incapacitating disease that decided many battles in the Pacific area. Fortunately, we did a better job of control than our opponents.

The troops also ran into diseases like scrub typhus in Malaya. This is a mite-borne disease which caused very high numbers of clinical cases. We had no drugs to treat rickettsiae, and scrub typhus is one of the rickettsial diseases. We now have drugs that are effective against those infections. So it seemed like every time troops moved into a new area, they would run into some new mosquito- or other vector-borne disease or a parasitic infection.

When they got to Okinawa, it was the first run-in with Japanese B encephalitis, and this virus infection caused an outbreak in civilian and military people there. It's a very serious disease, and the concern was that it was going to become a really rampant infection. Furthermore, we knew they would run into it immediately if they carried out an invasion into Japan. So it seemed like every time we sent people into one of these areas or into India, Nepal, etc.--any of those places where we sent troops--they were running into insect-borne diseases, and they were serious problems.

Hughes: There was the concept that these diseases were indeed united by the fact that they were arthropod-borne?

Reeves: That's right, because either a mosquito, a mite, or a tick--some type of an arthropod--bore them. For instance, in the African and Italian theaters, sandfly fever became a very important problem. That disease is carried by a phlebotomus fly, somewhat related to mosquitoes but a very much smaller so-called sandfly, which also carries leishmaniasis, another very important debilitating disease.

We sent troops into Panama to be trained in jungle warfare approaches, and immediately they ran into malaria, leishmaniasis,

and encephalitis viruses; they ran into a whole array of new viruses for which we had no vaccine, we had no treatment that was effective, and we had no preventive methods that were really effective. So it's no exaggeration to say that battles were won or lost in the Pacific and other theaters as much because of disease as they were from battle wounds. As a matter of fact, Dr. Tom Monath recently gave a summary of statistics on this problem to the National Academy of Sciences Committee on Microbial Threats to Health. He reviewed the actual casualties in major wars, and more troops were injured and incapacitated by diseases than by bullets in World War II and in all other wars.

Hughes: In all of those wars?

Reeves: In all of those wars. As a matter of fact, the current war (Desert Storm) that we had in Iraq is the first one we've had that was initiated and done before diseases could become a major factor as far as our troops were concerned. I don't know the data yet, but I know that even in that short, one-hundred-day war we're still going to have losses from diarrheal diseases, leishmaniasis, and maybe sandfly fever, and I don't know what other diseases.

Hughes: What about the specific need for entomologists?

Reeves: When World War II started, the armed forces recognized very early that they needed more entomologists because of the need to control insect-borne diseases. They very quickly pulled into the service almost all of the people who had such training. People who were well trained were used as instructors as well as to do control in the field. The people who were doing most of the entomological work had not been trained in either the university or on their jobs to work with mosquitoes, mites, or these diseases that occurred. They had to be retreaded entomologists who were given short, intensive training programs and then sent out to learn the hard way about their duties and the problems in the field.

Hughes: There was a controversy in tropical medicine over whether the way to control malaria was by eradicating the vector or by treating the patient. Was this an issue during World War II?

Reeves: I wouldn't say it was an issue. When you had a patient, it was too late; that person already had the disease. The reason you treated a patient was to get that person operating again and back on duty so they were not a permanent casualty. You also wanted to prevent them from being a source for further spread. With malaria, the troops themselves weren't necessarily the primary source of the infection. It was usually the natives in the area in whom the disease was highly endemic. Our troops got malaria



because they were exposed to the mosquitoes that had fed on the sources that were already there.

The treatment of patients in the case of dengue did no good whatsoever as far as preventing them being a source of infection, because we had no treatment for dengue that would eradicate the virus from their blood. In the case of Japanese B encephalitis, man wasn't even a primary source of infection for mosquitoes. So treatment of patients was primarily to get them back on their feet.

Eradication of a vector, for practical purposes, has very rarely been an effective approach to any problem in the history of this type of work. It's almost impossible to eradicate an insect from a sizable geographic area where it naturally occurs. The only time this has ever been accomplished that I know of is where an insect has been introduced from some other region, so it's not a native of the area. In those cases it has been possible sometimes to do eradication.

#### Vector Eradication Programs

Reeves: As a matter of fact, a major triumph in tropical medicine was the program that attempted to eradicate *Aedes aegypti* from the western hemisphere. Dr. Fred Soper was the head of this program for the Rockefeller Foundation and later for the Pan American Health Organization. Originally *Aedes aegypti* had been introduced into Latin America from Africa, so it wasn't completely adapted to its new environment. It was primarily in port cities, and it wasn't adapted to living in the jungle. So it was an urban mosquito living in households or on shipboard in stored water. It was a domestic mosquito. Soper was able to eradicate this mosquito from large areas of the Americas by almost a military type of operation such as [William C.] Gorgas did in Cuba. Soper's other success story was the eradication of *Anopheles gambiae* from Brazil. Again, it was an introduced species from Africa, and he succeeded in eradicating it in less than two years by focusing control on its limited range of breeding sources.

In contrast to that, the Rockefeller Foundation was planning to try to eradicate *Anopheles* mosquitoes from large areas, and they set up a project in Sardinia. Again, Dr. Fred Soper was much involved in this, and there were other people from the Rockefeller Foundation: Dr. Tommy Aitken, who is now at Yale; John A. Logan, an engineer; and Dr. Harold Trapido, another entomologist. They tried every way they could to eradicate the native *Anopheles*

mosquito that was the vector in Sardinia. They never succeeded in eradicating that mosquito.

Hughes: Because it was a native?

Reeves: It was just so well adapted to the environment that no matter what they did there was always a small residual population of the mosquito. They completely eradicated malaria from the island because they reduced the mosquitoes to such low levels that they couldn't transmit malaria anymore, but when they stopped the control program, the mosquito showed up again. It became widespread, and malaria was introduced again and became reestablished. So eradication has not had a very good track record. It's extremely difficult to do, and it's very rarely that attempts are made.

They are currently trying to eradicate the screwworm fly in Texas and Mexico because it's such an important pest in cattle. They think perhaps it has been eradicated in the United States, and now they're going to try to eradicate it from Mexico, where it's the source of reintroduction into the U.S. Whether they're going to succeed, I don't know, because it's a native species of the area. So eradication may not turn out to be a very feasible approach with the present methods and knowledge we have.

Hughes: The thesis<sup>1</sup> you lent me of the doctoral student, Randi Hutter, at Yale said that controversy apparently arose between the WHO committee and the Rockefeller people concerning the best approach to malaria control. The Rockefeller people maintained that vector control and maybe not eradication was the way to go; and the WHO people said no, as well as eradicate the vector, you've got to improve social conditions and educate people so they resist the disease.

Reeves: There wasn't unity in the staffs of either the Rockefeller Foundation or the WHO on what approach would be best. There were always people who had opposite views. WHO thought that by using DDT as a magic insecticide and by doing residual spraying of buildings where people lived, they would be able to reduce the *Anopheles* mosquito to such a low level that they would stop the transmission of malaria, and it would be eradicated. They weren't trying to eradicate the mosquito; they were trying to eradicate the parasite.

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<sup>1</sup>Randi Hutter. Dr. Wilbur G. Downs: Crusader Against Malaria. 1990. Thesis class of 1990. Yale University School of Medicine.

They soon found that in many instances their knowledge of the biology of the mosquitoes was completely wrong, because many good vectors of malaria would never go in a house and sit on a wall; they'd bite outside. Or they wouldn't sit on the insecticide, or they would become very reactive to the material so they would get away from it before they got a fatal dose. So they had to keep reorganizing their thoughts of how they were going to control malaria.

Now, I grant the fact that if you can improve the social structure of many of these populations as far as income is concerned and the type of housing they have, you will indeed to some degree minimize exposure to the vector. But none of these methods have really worked so far. The WHO's program to eradicate malaria from the world, in my opinion, was an absolute dismal failure in the sense that they oversold it. As soon as they stopped the program, the malaria parasites came back. Our knowledge of how to carry out eradications of a disease that is carried by an arthropod vector is still so incomplete that we really are not doing it at this time.

### Controlling Arbovirus Diseases

Hughes: A program should not be geared to eradication?

Reeves: I think it's more practical initially to try to reduce the disease to the point that it's not a major health burden on the population. I think in the end we're going to have a variety of approaches to accomplish this. Reduction of the vector is one way to get at it, improving the economic abilities of a population is another way, and immunizing the population is another potential approach. A lot of work has been put into developing a vaccine for malaria, but it is far from being anywhere near a success and accomplished fact at this time. Treatment of infected persons is the other approach. For a time they have had the idea that if they could put primaquine, which is a curative drug, into salt, then everybody who used salt would get their daily dose of primaquine, which would cure them of vivax malaria. This was believed to be a great way to get primaquine into everybody, because in many areas of the world where malaria is endemic, they import their salt; they don't have local sources. However, they soon ran into all sorts of problems in getting different doses of the drug--too much salt in some people's diet and not enough in others. They dropped that as a routine approach.



Any attempts to eradicate insect-borne or other arthropod-borne diseases, whether they're tick-borne, mite-borne, mosquito-borne, or tsetse fly-borne, in most cases will run into stumbling blocks. There will be a lack of enough knowledge of the biology of the insect which they're trying to control, or of the disease as far as its reservoir in nature, and other things of that kind. So it continues to be a real challenge. The young people now coming into this field say, "There are no challenges facing us." I say, "As long as man doesn't want to suffer from disease that's transmitted by arthropods, there's going to be a challenge, because we usually don't know how to do control effectively."

Hughes: Has there been a growing appreciation of the need to know about a vector's ecology in order to control it?

Reeves: There's a lot of lip service given to it. I don't know anyone who is concerned with controlling these diseases who will not say, "We need to know all we can about the ecology of the vector, because we're not going to be successful in any attempt to manage a population of vectors unless we know where they come from and what their basic life habits are. Then we can organize a control program that will hit them and be effective in reducing their numbers." It's so logical that nobody can argue about it.

They'll also say you have to have a very complete knowledge of the epidemiology of the disease, namely what population of people has the greatest risk of getting the infection. With that knowledge you can aim your control program at protecting those people at greatest risk from exposure and also determine what population is the most likely to get the disease once they're infected. Then we can develop a vaccination program to serve that population.

As long as you're trying to use all the different approaches that you can to control a disease, you have to know the ecology of the vector and the epidemiology of the infection in the population of humans who are at risk or are the source of infection. When you get to the arthropod-borne viruses which I work with, you also need to have detailed information about the avian or mammalian hosts--the animals in the environment that are the principal source of infection, particularly if man is not important as a source of infection for the mosquitoes or other vectors. That is true whether it's LaCrosse encephalitis virus in Indiana or Wisconsin, or Lyme disease in areas of California. Man doesn't make any difference in those infections; he's just a recipient of the infection, so we have to know about the animal hosts, the vectors, and their interactions.

Every time people have tried to use some shortcut to get around that sort of detailed knowledge, the program has failed. A current example is the genetic control of mosquito vectors, which we've made a five-year effort to accomplish in Kern County with *Culex tarsalis*. As I said, a lack of sufficient knowledge of the vector's mating habits was the reason for our failure in large part. We introduced the wrong population of mosquitoes, even though theoretically it would modify the population and reduce disease transmission. We didn't know how or where they mated, so we failed in our effort. So no, you can't have too much information. Even though everybody pays lip service to its importance, whether they put enough effort into getting that detailed information is another story.

Hughes: Is there something to be said about techniques developed during and after the war that were instrumental in promoting research on arboviruses?

Reeves: I think there was a big concern with development and discovery of insecticides that could be used to control vectors over large areas by airplane or ground applications. When you had hundreds of thousands of troops or populations that were out there being exposed to infection, you couldn't worry about controlling the vectors in a small lagoon or in a household; you had to control vectors over a large area. So when the invasion of Okinawa came, they felt very fortunate to have DDT and airplane application equipment. That island was completely covered with DDT applications from one end to the other before the invasion. They were going to do the same thing in some areas of Japan at the time of the invasion. So covering large areas with material such as insecticide to minimize disease risk was a major development. And indeed, at that time it was very successful.

Hughes: What about laboratory techniques?

Reeves: Laboratory techniques for diagnosis were important.

Hughes: How did you distinguish among arthropod-borne viruses?

Reeves: Actually, in areas like California it was obvious that we had cases, and we developed techniques that would identify a western or a St. Louis encephalitis case. In Okinawa we could immediately recognize that it was Japanese B encephalitis because we knew the disease was endemic in that area. During the war we had lists of the diseases that were anticipated in each of these areas before we were there. Bulletins of the clinical characteristics of the diseases, the diagnostic methods, and preferred treatment were put out for the medical and laboratory staffs. If there was a laboratory method, they tried to establish a backup laboratory in



the theater that would be able to do diagnostic work. It might be one that was moved up almost to the front lines; it might be at a base hospital behind the lines. So when Japanese B encephalitis occurred in Japan or in Okinawa, it was no surprise. We had anticipated it from what we already knew of the disease.

There were constant improvements in diagnostic processes, whether it was to identify parasitic diseases like malaria that were in the blood or doing stool examinations for schistosomiasis. And it was the first time the tests were applied to thousands and thousands of people who were not usually exposed to these infections. There was a lot of worry, for instance, when filariasis was encountered extensively in the Pacific, and our troops began to get it. It's a terrible disease. When the natives have it, they develop huge, enlarged legs, scrotums, breasts, and so on, because their lymph glands have been blocked. What we didn't realize initially was that this was because they had accumulated parasites in their lymph nodes over a period of time, which were blocking off the flow of lymph. When our troops got it, that didn't happen; they weren't getting enough parasites for long enough time to do that. Some troops were evacuated from areas the minute they were diagnosed as filariasis, because it was feared they were going to have all the various complications. It took some time to realize that the troops were not going to have chronic, repeated infections and the complications.

Hughes: Did you have a treatment for it?

Reeves: No, there was no treatment. The chemical prophylactic treatments for filariasis didn't come along until the 1950s, and even those haven't been too effective.

Virology was improving, bacteriology was improving, but really the only major breakthrough had little to do with diagnosis; it had to do with treatment. It was when antibiotics or other specific drugs for treating parasitic diseases became available. These were major breakthroughs.

When primaquine became available, that was the first time we'd ever had a cure for vivax malaria. But that was after the war; there was no primaquine during the war. All we had was atabrine, and everybody who took atabrine turned yellow. When I went to Okinawa in 1985, it looked like they had hepatitis. It was a yellow dye that accumulated in the skin. It was effective for suppressing malaria. It was a great improvement, much better than quinine, but everybody looked funny; they all looked yellow. Sometimes that made it difficult to diagnose hepatitis.



Hughes: I gathered from that same doctoral thesis that there was a problem with troop compliance.

Reeves: True. They'd have to line them up, put it in their mouths, and then make them open their mouths and prove they weren't hiding it there.

Another thing that happened as a result of World War II was that there was a great deal of concern that yellow fever might become an extremely important epidemic disease in some areas of the world, so they were very anxious to give vaccine to all the troops. Fortunately, Max Theiler and his associates at the Rockefeller Foundation had developed the 17D strain of yellow fever virus, which looked like an extremely effective vaccine. It would probably protect for life against that infection. There was only one problem, and that was that Dr. [Wilbur A.] Sawyer, who was director of the Rockefeller Foundation group at that time, felt from his and other people's knowledge of virology that the best thing to do would be to include human serum in that vaccine as a way of preserving the virus. Dr. K. F. Meyer argued against it, but they went ahead and added human serum. What they didn't realize was that hepatitis B virus would be in that human serum bank. As a result, they had many cases of hepatitis in the people who received the vaccine, and that was a very serious complication. It stopped when they took the serum out of the vaccine and put other stabilizers in it.

#### Civilian Advisors to the Military

Hughes: Did the military continue to rely on non-military people to conduct the research or provide the advice that it needed? Was that possibly another spur to research on arboviruses?

Reeves: I think so. During the war, for practical purposes all of the civilian population that had competence in these areas was in the military. Many of them were taken away from their area of scientific competence and put into some other activity.

Hughes: Was that deliberate?

Reeves: I don't think so. I think they were put wherever there was a vacancy and they needed a body to fill it. But they didn't see clearly that there was a need to keep most of these people in the area they were specialists in. I gave you the example earlier of Deane Furman, my roommate, who was well trained in medical entomology and parasitology. When he went into the military as a

lieutenant, they sent him to Fort Ord and put him in charge of the food services in the kitchen. He had to argue with them to transfer him over to the plague surveillance program at Fort Ord.

Hughes: Why didn't they do that in the first place?

Reeves: Don't ask me why the military does anything. They're a big organization, everyone was being mobilized rapidly, and they had to shove bodies into slots.

But in the postwar period the military took advantage of the fact that they had recognized many of these problems and their importance to the military, and that's when they developed an extensive group of civilians who were working for them or assisting them on commissions--the Armed Forces Epidemiological Board and so on. That activity started during the war, and when louse-borne typhus fever became an important problem in Egypt and Italy, they put together teams. Some of the members would be civilians and some would be armed forces people, and they sent these teams to Egypt or to Italy to study and control typhus fever or sandfly fever. They had what they called a commission on sandfly fever and a commission on typhus fever.

The Armed Forces Epidemiological Board became extremely active during and after the war to fill the need for extensive research on many problems, whether they were viral, pathological, or whatever they might be. Later, Korean hemorrhagic fever came up as a problem during the Korean War, and immediately they put together a civilian group with expertise to go to Korea to study this new infection that had newly emerged. They also sent military people and welded them together in a team.

What they were now doing was taking advantage of big laboratories that were available in the academic institutions, hospitals, and other types of civilian facilities to get contract work done to resolve many of the questions. So I would say that the war, again, brought to the attention of the military the importance of diseases to military operations.

They also were recognizing the fact that when you win a war, you immediately take on the responsibility for the health of a huge civilian population. You have a military government that is responsible for the health of the people in an occupied area. This then becomes a military problem. They like to forget that this is a problem they inherit. It's a problem that they've inherited right now in the Arabian gulf--thousands of refugees, thousands of displaced people, people without housing, and the military is assigned to take care of their health problems, whether it's emergency care in a hospital, housing them, feeding



them, preventing disease, or whatever it might be. An aftermath of any war is the occupation of territories and populations that aren't military people at all; they're civilian.

The Rockefeller Foundation, Arbovirus Research, and Disease Eradication##

Reeves: The Rockefeller Foundation before the war had a major program to study major diseases in various parts of the world. During the war, Dr. Wilbur Downs was immediately taken out of the Rockefeller Foundation program on malaria in Trinidad and was sent out to Guadalcanal to be a preventive medicine officer for [General Douglas] MacArthur in the Pacific. The people who were at the Rockefeller Institute and the Rockefeller Foundation were pulled out of working on yellow fever and other virus diseases and put onto malaria research. Dr. Harald Johnson, Max Theiler--all of these people were put onto working on malaria during or shortly after the war. They didn't make a lot of progress on resolving major problems, but they were doing good, basic research on malaria, because malaria was the major disease during the war, and yellow fever was not the major disease that had faced the military.

Now, when the war was over, certain people who came back to the Rockefeller Foundation realized that in various areas of the world, either due to the war or to other activities, there was a group of newly-emerging diseases that they hadn't been paying much attention to. During the yellow fever work in Latin American countries, they had constantly found new viruses. As they looked for yellow fever, they would find other viruses in Brazil, Colombia, Africa, or wherever they had projects.

So they knew there was a big group of agents out there waiting to be discovered, and they decided to put their major effort into what I called in one of my earlier sessions a head-hunting expedition. The objective was to find the full range of virus diseases that were out there, particularly in the tropical world but also in temperate areas. They believed a significant number of the viruses would probably be associated with a disease.

Hughes: Was the ultimate goal of the Rockefeller program to control the diseases or was it to find basic scientific information?

Reeves: I wasn't involved in their program planning, but I think their first objective was to get basic scientific information and a knowledge of the array of infections. Then they could get them



classified and put into some type of order. As it was recognized that those viruses were responsible for disease, then they would be concerned with initiating programs for control. I think that in this way they had a logical sequence of actions. They felt they had to know what the viruses were and what the other agents were if they weren't viruses--*rickettsiae* or whatever. Once they knew this and learned what the vectors and animal hosts were, they could build a base for further work on control programs.

You have to realize that the Rockefeller Foundation in this postwar period was very concerned with eradication of diseases. Malaria in Sardinia was a major project. Eradication of *Aedes aegypti* in the Americas was a major project; that was carried out by Fred Soper and company. They also considered eradication of cholera from the world. That was another ambition of Fred Soper. They had eradicated *Anopheles gambiae* from Brazil in 1939-1940. It was an introduced species from Africa, and it had become an extremely important vector of malaria in areas where it became established in Brazil. Fred Soper, as usual, did a dictatorial job of managing a program that eradicated that mosquito from the Americas in a two-year period.

Hughes: Is that because it was an introduced species?

Reeves: That was one of the explanations of why it was possible to eradicate it. That mosquito had extremely specific breeding spots in sunlit water. It was in the open, in shallow surface water on the ground. Once they learned that this specific niche was used by that mosquito, they could find where it was and wipe it out by using oil, DDT, or whatever approach was necessary.

Hughes: Was it their intention to eradicate it? Or were they just lucky?

Reeves: I think both. I think there had been enough experience that showed the Mediterranean fruit fly could be eradicated from Florida when it was introduced. One reason this was possible was that it was an introduced species, so it wasn't well adapted. I think Fred Soper understood biology well enough to think that because *Anopheles gambiae* was an introduced species from Africa and had very specific habits, it was a good target, and it was important as a public health problem to eradicate it.

Fred Soper believed in eradicating vectors and diseases. His idea was that economically it was the cheapest way to solve a disease problem. Reducing the disease was important, but if you didn't also eradicate the vector or the infectious agent, then you had to control it forever. He thought that was going to be much more expensive than putting a major push onto eradicating the vector and thereby the disease.

Hughes: Was he right?

Reeves: History so far hasn't proven him right, because we only know of one disease that's ever been eradicated from the world, and that's smallpox. Yes, that certainly was a cost-effective way to do it, and now we don't have to worry about eradicating or controlling smallpox again. We don't have to worry about keeping populations immunized against smallpox anymore. So in the long term it was the cheapest way to do it. But to find other diseases that are equally susceptible to eradication is the next problem.

### Vaccines and Vaccination

Hughes: How early on was it realized that finding vaccines for arthropod-borne viruses was going to be difficult?

Reeves: Yellow fever vaccine development was a very lucky break. They've never been able to duplicate the attenuation of yellow fever virus. Many people have attempted to do it. The attenuated 17D virus was a strain that showed up in the laboratory. Fortunately, it's potential was recognized. It's probably as effective a vaccine as any we've ever had--as effective, certainly, as smallpox vaccine. Smallpox vaccine is a live vaccine that immunizes a person probably almost for life, so once you get a person immunized you don't have to worry about him being a source or susceptible anymore. The same is true for yellow fever.

Dengue fever viruses also looked very promising in this regard. Albert Sabin developed a vaccine for the first two known types, dengue-1 and dengue-2 viruses, as early as World War II. It was a formalinized mouse-brain vaccine. What they didn't realize was that there were four dengue viruses, not just one and two, and that you'd have to have a vaccine for all four types.

One school of thought was that you have to have a series of exposures to dengue viruses before you developed hemorrhagic fever and shock syndrome. With that possibility you realized that you should have a vaccine for all four viruses. You didn't want to just make people immune to one or two and take the risk that they were going to get the complication of a fatal hemorrhagic disease when they got infected with the next type.

Hughes: Are they relatively stable viruses?

Reeves: The four types of dengue fever seem to be quite stable, yes. They don't seem to be developing significant variants. The armed forces have put millions of dollars into attempts to develop a dengue vaccine. Hammon spent a lot of time on it and on Japanese B encephalitis. Other people have spent time on it and still are. We still don't have what we consider to be a feasible vaccine. Some are now being evaluated for their feasibility, but we don't have in hand today a four-strain vaccine to be given simultaneously, as we do for polio, or any evidence that it would protect people against the disease. We also didn't know originally that there might be a difference in virulence between different strains of dengue viruses.

Dengue is probably the most important mosquito-borne virus in the world today. It is epidemic today in the Caribbean, Central and South America, and in Asia, in spite of all we know about it. We know the vectors, we know the viruses, we know how people get infected. But we really haven't solved the control of the mosquito vector. The reason is that people grow their own mosquitoes in their own households and yards, and they're very efficient at it--not intentionally, but they don't take care of the sources of water on their own premises sufficiently well to stop the mosquito vector from breeding there. We haven't succeeded in controlling dengue in any area where it is epidemic today.

When you get into the other viruses that are transmitted by mosquitoes, there are none in which man is the principal or sole source of virus for the vector. So if you had a vaccine, vaccinating a person could protect them against the disease; but it wouldn't stop the transmission of the virus, because the virus is coming from other animals, usually wildlife, that you're not going to be able to vaccinate.

Hughes: Was finding sources of viruses in nature a new idea?

Reeves: It came up in two places almost simultaneously. Yellow fever was the first discovery of the importance of different primates and other animals in the forests of Africa and Latin America. Work started in the mid-thirties with the discovery that there were jungle cycles of yellow fever in Africa and in tropical areas of Latin America that were completely independent of human infection. A new concept was developed that man was not the essential host for yellow fever; that man was an accidental host but could be an effective source of virus for an epidemic cycle from man to man by a domestic vector.<sup>1</sup>

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<sup>1</sup>See the oral history with Dr. Harald Johnson for more on this topic.



Then we came into the picture by our studies on western and St. Louis encephalitis, the Japanese came in with reference to Japanese B encephalitis, and the South Africans came in with reference to several viruses that were transmitted by mosquitoes in Africa. We began to realize there was a whole array of new viruses dependent on infection in wildlife. Then the Rockefeller Foundation international program was developed, which uncovered the fact that there was an almost endless array of such viruses in almost any place in the world that you started to look for them, and that you really were not looking for viruses in which man was an important host in the sense of being a source for vector infection. It was only important if the infected person happened to get ill. So that idea developed in the late thirties and the forties and expanded very rapidly.

Hughes: And of course that really affected the nature of control programs?

Reeves: That's right, because your approaches had to be completely different. I'm still arguing with people about whether a vaccine for western, St. Louis, or Japanese B encephalitis has any really effective place in our future control programs. My argument is that yes, it would be very good to have a vaccine, so that if you have a person working in the laboratory with a dangerous virus, you can immunize him and he would be protected from having an accident.

But when you go out to a free-living population of thousands of people, where the disease represents an accidental infection, unpredictable insofar as when these people are going to be exposed or if they ever will be exposed, then you realize that you'd have to vaccinate everybody in the population, or at least all the very high-risk people in the population, to have any effect on the human disease. Meanwhile, you have no effect on the basic cycle of the virus whatsoever. Anybody in that population who is not vaccinated is not going to be protected by what we call herd immunity in the surrounding people. As an example, in the opposite situation, with a man-man transmitted disease, if you immunize 90 or 95 percent of a population for measles, the remaining non-immunes are surrounded by immune people, so they don't have a good chance of coming in contact with an infected person. The same with polio. Actually, even that level of immunization hasn't eradicated measles or polio.

But with western, St. Louis, or Japanese B encephalitis, if you immunized 999 out of 1,000 people, it would prevent a lot of cases; but the one person who is not vaccinated is still at the same risk of infection as if nobody were vaccinated around him. You can say, "I still have protected 999," but at tremendous cost.

With most of the vaccines that we have at this stage, you'd have to reimmunize the people periodically; so you couldn't depend upon a lifetime immunization.

Most of the arthropod-borne virus diseases are going to be with us for many, many years. They're not going to disappear from our environment due to control efforts. People still talk about eradicating a lot of diseases: "Eradicate measles from the world." I don't think it's going to happen in the near future. "Eradicate cholera." I don't think it's going to happen in the near future. This is illustrated by the current extensive epidemic of cholera where it was reintroduced into South America. Cholera also reappeared in the Arabian peninsula just before the recent war started there. There are a few diseases that have been pinpointed that might be subject to eradication, but they're not necessarily the major diseases of our population.

Hughes: So has eradication largely disappeared from the agenda?

Reeves: No, there are still people like William Foege at the Carter Center in Atlanta, Georgia, and people at the Centers for Disease Control and people in medical practice who think this is a very logical approach and that in the long term it will be more economical to eradicate than to control a disease forever.

They're now trying to eradicate guinea worm from the Asian and African areas of the world. This is a worm that gets into and under the skin. The adults will be there, and when you walk in water, the guinea worm lays its eggs. Then that egg invades a copopod, a little crustacean that lives in the water, and goes through a cycle. So when people drink the water and the copopod, they get the infection. They say if we can control the copopods in the relatively few watering holes where people get their water during the dry season, we can eradicate this infection from the world. They are making a serious effort to do that right now in many parts of Africa, India, and other places where this infection is known to occur. It doesn't occur in the Americas. They could be successful, and if so they would have eradicated an infection which doesn't kill people but is a very bothersome one. But we have comparatively few diseases that seem practical to eradicate today. Polio is high on the agenda of WHO for eradication. Only time will tell if it is successful.

Hughes: When did wild birds become a factor in people's thinking?

Reeves: Wild birds came into people's thinking when they started to realize that wild birds or mammals--it can be either one--are the essential hosts for maintenance of many mosquito- and other vector-borne diseases.



Hughes: Was that a World War II concept?

Reeves: I would say that it arose before and during World War II from work like ours, the Japanese, and so on. But basically, it really came to full recognition after World War II; I don't think the war had much to do with the concept. People began to realize in this postwar period that they had a very interesting group of infections to study, even just out of curiosity, because of the very involved life cycles that they have. They also realized that these infections were much more common in many parts of the world than had been realized.

Many health agencies became interested in control, and there was research money to work on the arboviruses. This was why in 1952 I went out on a tour for the CDC, and I found some fifty different laboratories in the United States working on one or another phase of the arbovirus problem. It was an interest that arose during and after World War II and really became extensive since then. Plus the fact that the Rockefeller Foundation had field stations scattered all over the world that were turning up more and more of these infections.

### The Role of the World Health Organization

Hughes: What about the activities of the WHO?

Reeves: The World Health Organization was organized after World War II. The initial interest was in malaria, nutritional diseases, and problems like that. But they also began to realize there was a whole group of agents, which are called arboviruses, that were afflicting people all over the world, and WHO better have some interest in this. By the late 1950s, the Rockefeller Foundation program was full blown and was focused on these agents. They were putting pressure on WHO to have an interest in arboviruses, and WHO was recognizing that many governments of the world were becoming concerned about these problems. They had to get into the act, because that was a part of WHO's area of responsibility and interest. Governments would give them money and might say, "Arbovirus research is one of the areas we're interested in."

Hughes: What did WHO do that the Rockefeller wasn't doing?

Reeves: The WHO was responsible to governments, and governments were responsible to them, by international agreement. The Rockefeller Foundation was a private organization that really didn't have the



type of interrelationships with governments that the WHO had. The WHO is in many ways a political organization formed by countries. The Rockefeller Foundation is a philanthropic organization that is doing whatever the board of directors and the staff think is important. They're not doing what some country tells them it is interested in. That didn't mean they didn't share interests.

### Establishing Research and Communication Networks

#### **The International Congress on Tropical Medicine and Malariology Meeting, Lisbon, 1958**

Reeves: An International Congress of Tropical Medicine and Malariology was held in Lisbon in 1958. This organization began to have meetings in different parts of the world every three or four years after World War II. It would bring together people interested in tropical diseases. For some reason they kept the identification, Tropical Medicine and Malariology, I guess because malaria was and still is such an important disease.

This meeting in Lisbon probably was the largest international congress of people with these interests that had been held after the war. They had smaller conferences before that, but this was a huge meeting with probably over a thousand people there. It seemed like almost everybody in the world who was interested in arboviruses showed up at that congress. The Rockefeller Foundation paid a number of people's way. Almost all of the people in their international program were sent to the meeting. The Rockefeller Foundation paid my way to go to the meeting. Why they picked me, I don't know. The people from Russia came-- Smorodintsev and Chumakov. People showed up from various parts of Africa, India, South America--wherever arbovirus work was going on and had been for many years. So having that population there resulted in a number of symposia that were focused on arthropod-borne diseases. The Rockefeller Foundation and WHO representatives got together and said, "Let's have a special session at this time to see what the people in this field want to talk about as their problems and what sort of things they might share in the way of interests."

So they called this informal meeting, and it was interesting because it was co-sponsored by the WHO and Rockefeller Foundation. I went to it with the greatest of interest. However, I made a horrible discovery when I got there. I was pretty naive about

some things. They came up to me and they said, "We"--and that was the WHO and Rockefeller people--"have decided that you will preside at this meeting." Now, 1958--how old was I? I guess I was forty-two years old at that time. I had not gone there with anticipation that I was going to be asked to preside at anything. They said, "We've decided Telford Work," who at that time was with the Rockefeller Foundation, "will be the rapporteur for this meeting, and you will preside."

They didn't have an agenda organized, and there was no specific objective to be accomplished. The idea was just to get a group of people together who shared an interest and see what they wanted to talk about and what they thought their needs were. In retrospect, I'm sure that the real objective was to get a recommendation that the WHO should have an organized program in this area.

Hughes: Which they didn't have?

Reeves: Which they didn't have. They had nothing at that time in arbovirology. The Rockefeller Foundation did not want the responsibility for this. They felt that it should be some other agency. I think they felt that if they were to organize something, it would interfere with their objectives, which were quite different from WHO's.

Hughes: You're talking about responsibility for the research program, or more than that?

Reeves: Gathering information, getting information exchanged between different geographical areas and operations in different areas, different workers, getting a network of information available to the WHO, making sure there was centralized action when it was necessary. For instance, there had to be a centralized way in which virus identifications could be done. It just couldn't be done in everybody's laboratory.

Hughes: So the Rockefeller lab in New York wasn't sufficient?

Reeves: The Rockefeller Foundation lab in New York at that time was doing all it could, but it didn't have a network which was feeding it all the viruses of the world. They were getting what their laboratories were collecting. I would send them any new viruses I had because I wanted help on their identification, but basically there wasn't even an informal or organized network to do a lot of these things.

Hughes: So people really had no way of knowing what was going on around the world in arbovirology?

Reeves: We exchanged information at this meeting for the first time. I'll give you an example. The Russians kept talking about a certain type of mite that was a primary vector of viruses in Russia. It turned out there was a language barrier. They were talking about some sort of mite, but in fact they really were talking about ticks, and we couldn't cross that language barrier. We were talking about the same thing but just using a different vocabulary. A lot of vocabulary differences came up in these meetings that had to be resolved. We had the authorities of the world together as far as they and we were concerned, and the authorities of the world as far as I was concerned were sitting there talking about the same thing but arguing about it.

Hughes: Was there any consensus about how one goes about identifying an arbovirus?

Reeves: Yes, there was discussion about that. Also, there was agreement that there had to be an exchange of people and information so that we would be talking about the same things and know what was going on.

We had a tremendous argument at that meeting about the latest diagnostic methodology, which was the hemagglutination inhibition test for arboviruses.

Hughes: Why was that a source of argument?

Reeves: Because Albert Sabin had recently developed it. It was something that was modified from work that had been done earlier with influenza. He had people in his laboratory like Bob Chanock, who became one of the world's authorities on influenza later at NIH. He had had Ed Buescher in his laboratory, who later became the head of Walter Reed Army Institute of Research in Washington, D.C. Anyway, he had a group who developed this hemagglutination test, which was a significant addition to the neutralization and complement fixation tests.

The difficulty at this meeting was that a shouting match began between Ed Buescher, Albert Sabin, and Jordi Casals from the Rockefeller Foundation about the hemagglutination test--whether it was as good as or better than complement fixation and neutralization tests and small details on techniques. I was trying to preside, and these three people were all standing and yelling at each other at the same time. The Russians and some of the other international people were getting very confused by all this because they thought the Americans would all agree and take a unified front. I finally just slammed my hand on the table and I said, "Albert, shut up and sit down for a minute." Albert Sabin doesn't usually respond, but he shut up and he sat down. I turned



and I said, "Now, Ed, Albert has sat down and shut up. How about you?" He did. Then, without saying anything more, Jordi Casals sat down and just sort of smiled. He was a very nice Hispanic gentleman who didn't mean to be shouting in the first place and knew he was right.

The result of this meeting was that a consensus evolved very rapidly that we shared interests and recognized what the major problems were. These agreements were put together in a report which I don't even have a copy of anymore. It's possible that Tel Work at UCLA may have a copy; he was the raconteur.

The report went to the WHO. It stated that central or reference laboratories ought to be established that would cover the needs of the field laboratories all over the world to identify the virus strains.

Hughes: Were the locations specified?

Reeves: No, they were not specified at that time, but it also came out that there normally would have to be a central laboratory for the world, and it probably would be the Rockefeller Foundation laboratory in New York, which was later moved to Yale. It was obvious; they had the largest collection of viruses as a result of the early work on yellow fever and associated viruses in Africa and in South America. They had field laboratories established all over the world that were feeding new material to them. No other country and no other single facility had anything like that. The WHO is not a research organization in the sense of establishing a laboratory in Geneva to do this.

About the same time, it was recognized that there were these other laboratories that could serve regional areas, such as--

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Reeves: --the CDC laboratory, which is now at Fort Collins. The Moscow laboratory of Chumakov was one, and laboratories were to be established in Latin America and in Canberra, Australia.

Hughes: Were these labs established rather rapidly?

Reeves: Fairly rapidly after that, because the WHO then said that these would be their officially-designated reference resource laboratories. They provided token funding, and I do mean token. They didn't supply a working budget, but they supplied a small budget that would allow shipment of materials back and forth, sending out of reports, summaries, and things like that; but they

didn't supply staff. This had to be done by federal governments or by foundations like the Rockefeller Foundation.

WHO facilitated visits between workers in different laboratories, because the WHO had the ability to get visas and to get government approval for people to move across lines that sometimes governments couldn't or wouldn't necessarily endorse.

Also at that time, the recommendation was made that the Pan-American Health Organization should become involved. Most people don't realize the Pan-American Health Organization is not a part of WHO. It was formed before WHO by Fred Soper and company, and they feel they are autonomous; they're responsible to the governments in the western hemisphere, and they're not responsible to WHO. WHO likes to pretend that they're part of it, but basically PAHO insists on its autonomy, and their agreements with governments are quite separate from the WHO agreements. They generally work together; however, sometimes they work in opposition. I really don't want to get into details, although I ran into it several times.

These two international agencies had to be willing and able to help open doors to get materials and personnel back and forth.

Hughes: But they were not actually doing the work?

Reeves: Well, generally not, although the Pan-American Health Organization did establish some laboratories in the western hemisphere that included arboviruses in their activity. They had a zoonoses center in Argentina, and they established a laboratory in Venezuela that did some arbovirus work. Sometimes they put arbovirus research into agricultural laboratories that were working on hoof and mouth disease or other virus diseases of animals so they could utilize some of the same people and equipment.

We also agreed in Lisbon that, in addition to visits between the field and central laboratories for training or for exchange of information and techniques, there would probably be a need for training programs for people who were going to be coming into the field, and that there was going to be a need to develop laboratories in various parts of the world where there were none at that time. We agreed that the study of migratory birds would be important, because it was increasingly recognized that birds and other animals moving from one area to another could be moving these viruses around. So a whole series of rather specific studies were recommended; it was a very ambitious project proposal.

Things moved very rapidly, because in November of 1958, after the Lisbon meeting, the first WHO Scientific Group on Virus Diseases met in Geneva. They had such an organization because of their basic interest in influenza, polio, smallpox, and a lot of other virus diseases. They rapidly ratified the recommendations of the Lisbon meeting. They had other advisory groups that dealt with immunological surveys and so on, and they also agreed to the arbovirus proposal.

The virus commission meetings [of the Armed Forces Epidemiological Board] were always held in Washington, D.C., because we met at Walter Reed Army Institute of Research. In 1959, at a commission meeting, there was a large group of us there who were concerned with arboviruses and who were also consultants to WHO, the NIH, or the army, and we realized that WHO alone was not going to take care of all the problems. So one night we got together to see if we couldn't get something organized in the way of a working group in the United States that would concern itself with such problems.

Now, the difficulty was that some of us worked for universities, others worked for the Rockefeller Foundation or the army. We represented a variety of different organizations. We didn't have an interchange between us that was organized in any sense, and we decided that with a little bit of support from some agency like the Rockefeller Foundation we could organize some meetings and get us together again on a completely voluntary, unpaid basis. We had enough interests in common that maybe we could start implementing some of the recommendations made to WHO. That really was the beginning of what later became the American Committee on Arthropod-borne Viruses.

#### The Gould House Meeting, 1959

Reeves: The outcome was that the Rockefeller Foundation was very eager to have the recommendations implemented, so they called a meeting very shortly after that in 1959 at what was called the Gould House Meeting; The Gould House is a meeting place in Ardsley-on-Hudson that the Rockefeller has available to them. They organized a group of nineteen of us scientists and administrators to meet there to discuss how we might implement some of the recommendations that had been made, not in an organized fashion in the sense that we were going to be put together as a research unit to do this, that, and the other thing, but to consider the possible approaches to the problems.



Hughes: Just American workers?

Reeves: They were all American workers, but some of these people represented the international projects of the Rockefeller Foundation. So what that group agreed would be most helpful was an exchange of current information on what was going on in research in this field. We had to have some way of finding out what this guy is doing and that guy is doing, so we would complement each other and learn from each other. We agreed that, given an opportunity, we would make all our information available to everybody else in a completely voluntary fashion.

Hughes: Was there any hesitance?

Reeves: No, there didn't seem to be any. It was a very interesting group of people. The people who were in that original group represented a wide variety of organizations. It was almost unbelievable. Sure, they argued about a few things; it wouldn't be any fun if you didn't. But we had people from the Rockefeller Foundation, the U.S. Army, U.S. Navy, U.S. Public Health Service, three universities, and one state health department.<sup>1</sup> We came to agreement on all the things that we could do.<sup>2</sup>

#### The Second Meeting of the Gould House Group, in Chicago, 1960

Reeves: That seemed like a good beginning, but it just wasn't going to go on without something else, so we got them to call another meeting in April, 1960, in Chicago at an international meeting. It was a follow-up meeting to pursue the Gould House recommendations. At that meeting they recommended there ought to be a formal organization meeting to be held in Atlanta, Georgia, within a relatively short period of time. The Rockefeller Foundation funded the meeting in Atlanta, Georgia, in April, 1961. It had a

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<sup>1</sup>Rock. Found.: C. R. Anderson, J. Casals, D. H. Clark, W. G. Downs, R. Morrison, K. C. Smithburn, L. Whitman, T. H. Work, M. Theiler. U.S. Army: E. L. Buescher, J. E. Smadel. U.S. P.H. Serv.: R. W. Chamberlain, C. Eklund. Univ. Pittsburg: W. McD. Hammon. Univ. California: W. C. Reeves, R. M. Taylor. Cornell Univ.: W. F. Scherer. Calif. State Dept. Health: E. H. Lennette. U. S. Navy: H. Hurlbut.

<sup>2</sup>Form a Subcommittee on Information Exchange, a Subcommittee on Reagents, and recommend that the Rockefeller Foundation in New York should function as a Central Reference Laboratory.

very interesting outcome, because we decided there should be an organization to be named the American Committee on Arthropod-borne Viruses (ACAV) with the charge to implement research in this area. We even agreed that if Rockefeller Foundation provided some funding, this organization would be able to get information disseminated and have a small budget, not to hire people but to pay for miscellaneous expenses.

Now, I again got drafted into being the chairman of that Atlanta meeting; I don't know how or why, but I was. They agreed at that meeting that they wanted to formalize the group, not to the extent of having an organization that would be incorporated but that would have dialogues and so on. They would have a series of working groups of people concerned with certain problems, and I could serve as chairman and treasurer for this informal organization.

Meanwhile, the WHO wasn't sitting still. They put together an organized study group on arthropod-borne viruses in 1960. That was a pretty rapid development for WHO. Within two years they organized a group to put together their views on the total field and to review the recommendations of the Gould House, the Lisbon meeting, and all of these things. The WHO group specifically endorsed and supported all the recommendations that had been made. Again, it was an international group<sup>1</sup> which represented all the countries of the world that had a concern with arboviruses. As a matter of fact, they had too many people to fit in as members of that committee. The WHO is limited in how many people they can have from any one country.

Dr. T. Work and I were drafted to be in the secretariat of the organization. I'd never recommend to somebody that they do that unless they like to work, because what happens is that the major members of the committee sit and talk, and the secretariat puts it all together and makes sense of it. We had an excellent group of people. We worked very hard day and night.

A report came out of that committee which emphasized that arthropod-borne viruses represented a recognized field of effort.<sup>2</sup> It recognized that there were both official approaches to the problem by organized groups like the Rockefeller Foundation

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<sup>1</sup>Dr. R. M. Taylor, U.S.A. (chairman); Prof. D. Blaskovic, Czechoslovakia (vice-chairman); Dr. J. D. Gillett, Uganda; Dr. H. Groot, Colombia; Dr. J. A. R. Miles, New Zealand; Prof. A. K. Schubladze, U.S.S.R.; Dr. C. E. Gordon Smith, England; Dr. H. Trapido, India; Major E. L. Buescher, U.S.A.

<sup>2</sup>WHO Arthropod-Borne Viruses--Report of a Study Group. World Health Org., Tech. Rept. Service No. 219. 1961, pp. 1-68.



worldwide reference center, which would soon be at Yale, and the regional laboratories that would be developed. ACAV activities would include a newsletter, to be developed by the American group, that would not be limited to news from the United States; it would collect information from all over the world from anyone who wanted to submit information, and it would not be a publication that could be cited as a scientific reference source. Also, meetings would be held in association with major international and national meetings of scientists concerned with virological and tropical disease research. There would be enough people interested in this particular group of agents to have meetings held within formal meetings and congresses. This would allow arbovirologists to be kept up to date on what was going on.

#### The American Committee on Arthropod-borne Viruses

Hughes: Did any other branch of virology at this time have a similar organization?

Reeves: No, and they still haven't. This group we call the ACAV--some people call it "A-CAV." I don't like that, because it sounds like a sick calf or something. Regardless, this group will always be thought of as a unique one. Other organizations concerned with groups of viruses or diseases get very official; they are incorporated, have all sorts of rules and regulations for developing virus nomenclature, and so on and so forth. They aren't free, cooperative endeavors; usually they wind up as a battleground for disagreement more than agreement.

All of the ACAV activities represent free labor, for practical purposes, and boondoggling for funds to pay for something. Private, federal, and state agencies have no idea how much they have contributed in the way of volunteer labor, time, and money. The group puts out a worldwide newsletter, and they developed a catalog of the arboviruses of the world.<sup>1</sup>

To get a new activity organized, the current chairman will be presiding at a national meeting and will say, "Look, here's a subject in which we don't have all the information we need. Who's interested in it?" A hand will show up here and a hand up there, and they'll say, "Okay, you're the committee. Go have a meeting, tell us who your chairman is, and tell us what you think the problems are in this area," and they go and do it. It's a free

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<sup>1</sup>R. M. Taylor (Ed.). Catalogue of Arthropod-borne Viruses of the World, 1967. Pub. Hlth. Serv. Publ. No. 1760, U.S. Govt. Printing Office, Wash., D.C.



association of investigators. If somebody doesn't like what's going on, they don't have to stay, though I don't know anybody who's withdrawn because of unhappiness. It's just an amazing voluntary getting together of people in the field, and I think it's unique. It's really unique in terms of accomplishment and the amount of clout the group has had scientifically.

ACAV developed a nomenclature for arboviruses. Now the molecular biologists are developing independently a completely different type of classification of these viruses based on molecular characteristics and so on. The ACAV's concern is not that molecular grouping is unimportant, but our grouping is based more on biological and epidemiological considerations, which frequently cut across the molecular construction of the viruses. The two approaches complement each other, although there are times when they seem to get in each other's way. I don't think there's a real conflict; I think both are needed. So the ACAV tries to utilize the molecular approach as much as it can be a part of the biological knowledge of arboviruses, but not as a controlling, overruling factor.

It's been a very interesting organization, and it's been very interesting to see new committees develop over the years. I suppose there have been probably a dozen different committees. Some have completed their tasks. The veterinary group met, and they developed topics for meetings at the tropical medicine meetings. The ACAV has become associated officially with the American Society of Tropical Medicine, although they also meet with other agencies and other organizations. The veterinary group had meetings and did what they thought was necessary, and then they just abolished themselves. The committee on wild bird movement was a very active group for a while, brought all the information in the area together, and then they abolished themselves. There wasn't any need for them anymore.

ACAV has developed to the point where it has worldwide recognition. Organizations responsible for world health problems accepted this organization, and it dictated to some agencies things that were needed in this field. For example, we needed new reagents. Now, you can't make reagents to identify viruses and classify them without money. Enough pressure was put on the National Institute of Allergy and Infectious Diseases that it formed a special committee to guide its efforts in preparing reagents to be stockpiled and put in the depository of the National Type Culture Collection.

The National Institute of Allergy and Infectious Diseases gave contracts to people to make reagents, and the committee that was advising them consisted almost entirely of members of the ACAV. But they were not representing the ACAV; they were representing scientists who were concerned with this problem. The

National Institute of Allergy and Infectious Diseases had reference reagents made that were stockpiled and distributed all over the world, and the NIH paid for them.

Hughes: Remarkable.

Reeves: Once that job was completed, we abolished that committee. Wilbur Downs was the chairman of the first committee, and almost all of the people who were really knowledgeable in this area were on it. That was fine, except that when the National Institute of Allergy and Infectious Diseases appoints a committee, the people can only serve for three years and then have to be replaced. At the end of three years the Institute suddenly came to the realization, "Hey, these people have served out their terms, they can't be renewed, and we've used up almost all of the key people."

Hughes: So what did they do?

Reeves: Karl Johnson had just been put on this committee. He was an employee of the Public Health Service stationed in Panama, so he didn't have a terminal date. So when they finished their meeting, Dorland Davis, the head of the National Institute of Allergy and Infectious Diseases, told the committee that the next meeting would be their last meeting, and thanked them for their services. NIAID then had to get new members, including a chairman.

They sent Karl out to Berkeley to get me to agree to be the new chairman. He'd never been here before. They had decided they wanted me to be the chairman of this committee to replace Wil Downs. I said, "Reagents aren't my field." Karl said, "We'll all help you, and you can do this all right." I said, "I'm already on another committee for the National Institute of Allergy and Infectious Diseases, and you can only be on one committee at a time." He said, "What committee are you on?" I said, "The Epidemiology and Biostatistics Training Committee." He said, "Do you want to be on that committee, or do you want to be on this one?" I said, "I'd be very happy to get off that committee. I'd much rather be on this one." So I used that as a way to get off the training committee, because I wasn't finding it very interesting.

Then we had to get together a new group of people that could be advisors on the reagent committee. We were able to find enough people to fill out the committee and to complete the task, and then we abolished the committee.

Again, this had been a matter of getting the WHO and other organizations to endorse the ACAV recommendations that reagents were needed so that we could convince the NIH to do it. If we



were going to do it again today, it probably would still be done by the National Institute of Allergy and Infectious Diseases, because CDC doesn't have the contract and research grant capability to do it.

At the same time, we were putting pressure on CDC to develop broadened programs in arbovirus research. There were a whole variety of activities that could be stimulated by the ACAV.

### The Arbovirus Catalog

Hughes: Do you want to say anything about the procedure for registering a new arbovirus?

Reeves: There's an ACAV committee that deals with the registration of new viruses. This is headed up currently by Dr. Nick Karabatsos. He's at the CDC Laboratory at Fort Collins. We've had a series of chairmen. There is a form for registration of a new virus that usually is completed by the person who's isolated the virus. A subcommittee of ACAV reviews the information for its validity and determines whether the virus can be put in the catalog. But meanwhile, a tentative registration is made and distributed to the various laboratories that would be interested in it; it's not put in the permanent catalog immediately.

The person who gives the catalog registration may be asked to do certain things to complete that registration. Once it's in the catalog, that person usually is responsible for maintaining its current content. I, for instance, inherited the problem of western equine encephalitis. Dr. K. F. Meyer was the original person responsible for it, but he wasn't available to register it and keep it up to date. If there's going to be a new catalog, I have to review that particular card and make sure it's brought up to date.

Hughes: Are all the cards covered in that fashion?

Reeves: All the different arboviruses known in the world, for practical purposes, are in there, except there are new ones being added constantly.

Hughes: Was the classification system preexisting?

Reeves: Partially, due to the work of Jordi Casals of the Rockefeller Foundation. He, on the basis of complement fixation, had separated the viruses into what we call alphaviruses,



flaviviruses, and so on. And then the other classifications that evolved since then were based on either serological or morphological (molecular biology) type of markers. So much of the classification system now is based on the work of other committees that deal with nomenclature and taxonomy of all viruses, not just arboviruses. They are working independently from the ACAV. We try as much as possible to follow the classifications being used for the viruses in today's world. This activity has resulted in the publication of three editions of a catalog of the arboviruses of the world,<sup>1</sup> encompassing over five hundred viruses. The catalog is disseminated worldwide to libraries, educational institutions, and research laboratories. Committees have voluntarily reviewed the status of each virus: to determine whether it is transmitted by arthropods, its basic epidemiology, whether it causes disease, its antigenic and molecular characteristics, wildlife hosts, etc., etc. Nobody is required to register a virus, and the people who review these viruses as far as their status is concerned do it all on a voluntary basis; no one's paid to do it.

Membership in the ACAV is the most informal thing you can imagine, because all that's required to be a member is to go to an American Society of Tropical Medicine and Hygiene meeting or some other organized meeting of ACAV and sign a piece of paper saying that you've been there and, "I want to be a member." That's it. Now you're a member if you continue to attend meetings.

Hughes: No money?

Reeves: No money. No dues. The original budget was \$10,000, which I was given by the Rockefeller Foundation back in the late 1950s. I was treasurer for years. I think I turned over almost \$5,000 to Dr. Tom Yuill at Wisconsin when he became the treasurer. I didn't want to be the treasurer forever. The members always said I was too tightfisted, but at least I had something to turn over when I quit.

Hughes: General Russell mentioned your putting money for the ACAV into an account here at the university.<sup>2</sup>

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<sup>1</sup>N. Karabatsos, Ed. 1985. International Catalogue of Arboviruses, 1985. Including Certain other Viruses of Vertebrates. Published by the American Society of Tropical Medicine and Hygiene. Third Edition. pp. 1-1147.

<sup>2</sup>Telephone interview with Philip K. Russell, April 1, 1991.

Reeves: Originally it was given to the university to be managed by me as the treasurer for the ACAV or as its chairman, whatever I happened to be at the time. It was just an open account from the Rockefeller Foundation to the regents, to be administered by me, purely to support the ACAV activities.

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Hughes: Was it enough money to support endeavors such as the newsletter and the catalog?

Reeves: No. As a matter of fact, we usually didn't spend money from the treasury for that sort of thing. We usually bootlegged it some other way or obtained a grant.

Hughes: What do you mean by that?

Reeves: If somebody wanted to put out a newsletter or something like that, and they were with an organization that had a facility that could do it, the organization didn't necessarily have to know they were paying for it.

Hughes: You couldn't get away with that for a whole catalog, could you?

Reeves: We have for practical purposes, although a number of organizations have knowingly contributed funds for the catalog. Besides having a catalog, we put together all the abstracts on arboviruses that came out in Biological Abstracts and at least two other abstracting services. We got permission from those services to duplicate, catalog, and distribute them. So anyone who received the arbovirus catalog also received files of the abstracts. There are endless boxes of these now, and they're all classified in categories so you can get your hands on them by virus or by some other key.

Hughes: Why does the catalog include some viruses that are not arthropod-borne?

Reeves: Because they're associated with research on arboviruses. They were discovered as a result of arbovirus research programs and have a basic cycle in an animal host, even though they do not have an arthropod vector. Not infrequently they may have a serological relationship to viruses that do have a vector.

Hughes: Isn't that confusing to have them included?

Reeves: Not necessarily, because we put them into a different classification, and we say they're not arthropod-borne. A lot of viruses are in there now that are not arthropod-borne--some of the

rabies-like viruses and the hemorrhagic fever viruses. But they're always associated with wildlife hosts, even though they don't have arthropod vectors. They usually were discovered as a part of a study that was intended to be on arboviruses, and the virus was originally thought to be an arbovirus; then the virus was studied and found not to be carried by arthropods.

Rio Bravo bat salivary gland virus that Harald Johnson discovered here in California is a good example. It's transmitted directly from bat to bat without an arthropod vector; but it's closely related to St. Louis virus antigenically, so it's a flavivirus. How are you going to classify it? You can make isolations of this virus from some bats, and you find it's a flavivirus; yet you have to differentiate it from St. Louis virus. Modoc virus from *Peromyscus* mice is the same sort of problem.

Some of the viruses that are not arthropod-borne are closely related to arboviruses, so we have those in the arbovirus catalog. Where else are you going to put them? You can't ignore them. If you have an information service on arboviruses, you'd better know every virus that's closely related antigenically. You also want to have the viruses that occur in similar environments and that have almost the same type of basic maintenance cycles. They may be transmitted directly from animal to man or animal to animal without an arthropod vector, but they're still in the same environment and take the same type of research approach. So we decided not to let them be orphans, but we put them into a different classification system so that they stand out.

We make no bones about it in the catalog. We put "Not arthropod-borne" on the catalog card. One of the committees makes decisions on whether the evidence shows a virus is not arthropod-borne, or if the evidence shows it is arthropod-borne, or if it's not known yet. We still don't know how some viruses are transmitted.

Hughes: When a new edition of the catalog is put out, is each entry assessed?

Reeves: It's brought up to date. So the editor has a tremendous responsibility to badger the various registrants to do that and then to make sure by checking the literature that something important hasn't been left out. The editor may even have to refer whole groups of registrations to committees to review and to double-check that everything is right.

Hughes: Is there anything to be said about the WHO study groups?



- Reeves: Only in the sense that they cover a wide variety of subjects. Some of their groups are no longer active or have been combined. I am still notified each year that I am a consultant in virology to the WHO and to PAHO, but I have not been involved with a committee of those agencies for a number of years.
- Hughes: What about the Gould House committee? It was the predecessor of the ACAV, right?
- Reeves: Yes and no. The Gould House committee was a selected group of people who tried to get something off the ground. But when the Gould House committee was disbanded or simply didn't meet again, it decreased considerably the degree of involvement of the Rockefeller Foundation. However, the people who worked for the foundation still were involved in the ACAV. They no longer called the meetings or funded the meetings and so on. So they just disbanded. There's no longer a Gould House committee. Nobody even knows who was on this except you and me and a few other people who were on it and are still alive. You have to remember that the committee met in the period 1959-1961. A lot of these people aren't here anymore. For example, Drs. Clark, Downs, Hammon, Taylor, Anderson, Buescher, Theiler, etc.

### The Arbovirus Newsletter

- Reeves: The Arbovirus Information Exchange is an ongoing committee. The current chairman, Dr. Charles Calisher, puts out the Arbovirus Newsletter two or three times a year. It's also done by free labor and actually paid for and mailed by CDC. The CDC gets the worldwide unpublished information on research, epidemics, etc., as a payoff.
- Hughes: Does the newsletter go to everybody who's ever shown an interest in arboviruses?
- Reeves: If they request it, number one; but also they must contribute information with some regularity. People are dropped from the newsletter mailing who don't make a contribution at least once a year. The reason for that is that there was a bunch of freeloaders who wanted newsletters, and they didn't know what the heck to do with them. It was free, so they asked for it. We wanted to have people who were going to do something with them and provide information. A person in WHO might not have any research data to report, but he could take the responsibility for reporting what WHO was doing in the region, make sure his name appeared on something that came out in the regional report. But we don't owe

them anything. WHO doesn't get a nickel from us or any piece of paper without earning it, because we're not working for WHO.

Hughes: Is that a bone of contention?

Reeves: No. But I don't think there's anybody in WHO in Geneva who is a regular contributor. I'm sure a copy of the newsletter goes to Geneva without them making a contribution in the sense of information, because certain governmental agencies will get it. But we want to make sure that the newsletter represents the workers--an exchange of information from them. We're not nearly as concerned about people running programs knowing about this. It's the job of the people working for them to keep them informed. It is not a publication.

Hughes: How do people protect the information that goes in the newsletter?

Reeves: No editor of a respectable journal will accept the newsletter as a reference source. If they did use it as a reference source, they would have to have a personal communication with the person who submitted the information. Any reputable journal would do that.

Hughes: Is it your understanding that people are very free about the information they provide for the newsletter?

Reeves: Yes. There are a lot of things that appear in the newsletter that never appear in publications for a variety of reasons. Some of it is worth publishing, some of it's gossip, some of it's good data that may already be in print. We don't want the newsletter to be competitive with publications. That's not our purpose.

Hughes: Do people submit material to the newsletter that they are going to publish eventually without fear that it will be taken?

Reeves: Yes, because it's not a publication. It's very clear.

Hughes: How long do committees continue?

Reeves: I mentioned earlier that many committees we have had are now disbanded. We had a committee on laboratory infections in which we were trying to determine how much risk there was if one of these viruses infected a person who worked with it in the lab. We've had a few bad accidents where people have been infected and died. That committee periodically reviews what laboratory infections or experiences have been had by people working in laboratories. So you got information on what proportion of people who worked in these laboratories became immune from having inapparent infections and how many had an associated illness.

Now, that basic data we have collected is used by the CDC to classify the risk of different viruses and classify the type of laboratory facility required to protect the workers. They are called class 1, 2, 3, or 4. In class 1 you can work on a benchtop with the virus; class 2 is what we call an open laboratory, but still with hoods that offer protection. A class 3 laboratory means that you have to be restricted as far as visitors are concerned, work in safety hoods, and so on. There are very few class 4 laboratories, and these are at the CDC, the army, or the U.S. Department of Agriculture, and they have maximum containment, even wearing space uniforms in some. Class 4 means that they are working with highly dangerous organisms that we don't want people who are not immune to be exposed to, or we don't want to take a chance of releasing them, as many do not occur in the United States, but we still need information on them.

#### Immunizing Laboratory Workers

Hughes: Does the committee take a stand on mandatory immunizations for laboratory workers?

Reeves: The committee can't take a stand on that because we don't have the authority to take a stand. We can make a recommendation. It is up to an institution whether it will have mandatory immunization or not. As an example, at Fort Detrick they try to make a vaccine to protect their people against every high-risk virus or other agent that they're working with in the laboratory. This protects the individual and decreases the chance that a virus will escape. They are willing to make many of these vaccines available to people outside of their laboratory.

At Berkeley I ran into a real problem when I wanted to use vaccines they had developed for a few of the viruses that occur in California or that we were working on in our class 3 laboratory. The problem was that the liability for administering the vaccine comes under the medical services for university employees at Cowell Hospital, and they would not touch the vaccines because they were not licensed products available to the general public. They weren't satisfied with the evidence that they were safe from any side effects. Cowell Hospital staff said, "We won't take this responsibility. We will not administer this under a medical service program for university employees."

Hughes: So you don't require immunization?

Reeves: We don't require it, obviously.



Hughes: Do you give it?

Reeves: No, we don't give it. We have given some of these vaccines to people who volunteered or requested them at times, but we try to avoid working with what I would consider a maximum containment virus--I mean, really maximum. If we isolated one as soon as we knew it, we would refer it to a class 4 laboratory and not keep it here.

Hughes: How is St. Louis virus classified in terms of risks?

Reeves: It means that you have to work under hoods with it; you have to use fairly stringent safety measures. Your lab is closed to non-trained persons. We don't have a vaccine for that virus, and it is worked with by very few persons behind locked doors for safety reasons only.

Hughes: Have you had any problems with laboratory infection?

Reeves: We haven't, but other people have. Last time I reviewed our records, we didn't have any evidence of current laboratory infections in our people. We take periodic blood samples. We have had possible exposures with a needle stick but no detectable infections.

Hughes: Have we said enough about arbovirus work elsewhere?

Reeves: I don't think there's really much point in further discussion. I may know a lot of what's going on at a lot of places, and I'm not saying I can't discuss it because it's classified or anything. It's just a matter that I would be afraid that I would start talking about things and not be complete or correct.

### Biological Warfare

Hughes: Can you give an example?

Reeves: It's obvious that the U.S. Army Medical Research Institute of Infectious Diseases at Fort Detrick is concerned with work on agents that are of particular risk to armed forces personnel who go into various parts of the world and might be exposed to an infection that does not occur in the U.S. or one that was released by the forces of another country. So they are concerned with any agent that someone else might use for biological warfare.

Hughes: Or we might?

Reeves: That is not a very considerate question. The charge to the Fort Detrick laboratory is to develop a defense against such possibilities. No work is done to develop a capacity to use such weapons, but very sophisticated research is done to develop new and effective vaccines and to learn how such diseases are transmitted in areas where they occur naturally and might be encountered. There are very valuable spinoffs for the general public from this work, both in providing protection from diseases with new knowledge and by new vaccines becoming available to the public. The laboratories are under strict congressional orders and international agreements that this country is not to develop any agents for biological warfare purposes. There's an international treaty on this that we, Russia, and many other countries have agreed upon. That's all I'll say about it, except that anyone who followed the news media on the last war, Desert Storm, knows this was an area of concern, and we were well prepared to defend our troops in the event that a biological or chemical agent was released in Iraq.

I also can say with a great deal of confidence that I have never, in all my relationships with Fort Detrick or other armed forces activities in the last forty years, had any reason to think they were developing or planning biological warfare offensive capability--none whatsoever. The same is true for the Navy Biological Laboratory that our school was associated with for many years. As a matter of fact, I have very strong feelings myself that most biological agents would be very poor warfare weapons. There are much more efficient ways to achieve the objectives of a war.

Hughes: Why do you say that?

Reeves: The difficulty in using a biological agent is that you have to be awfully sure you can protect your own people from it and also be very sure that you're not going to put something into place that you don't want to have to live with for the rest of your life and be responsible for the lives of human beings. I just don't think this is a very profitable or efficient way to develop warfare. Other people may disagree with me.

I know Fort Detrick is working on viruses that occur in Africa, Latin America, or Asia. The reason they were assigned this task is that they think these diseases will be a threat to troops if the U.S. government puts large numbers of people there under wartime conditions. Or they might be particularly dangerous agents if they were introduced into the United States accidentally or purposefully. The U.S. Public Health Service has taken a very

similar position, although their focus may be on the protection of a civilian population. The various branches of the U.S. government must maintain research and develop a knowledge on many diseases that do not occur in the United States. It is in our best interests.

Hughes: I would think that your work on the committee concerning the emerging viruses would be focused on some of these problems.

Reeves: The committee is considering both infections that occur here now and those that might be introduced and is trying to put the problems into proper perspective. I'm not on the subcommittees that will be considering many of these problems. I will be involved in the final agreements and preparation of the final report. The areas and problems being considered are almost endless, but none can be ignored. We have to consider the possibility of genetic change in agents, that new agents may emerge. You have to consider that someone might try to develop an agent for warfare, what the probabilities are that it would be successful if they did, and what the risks would be. It is far too early at this stage to discuss this problem further until the final report is prepared and available.<sup>1</sup>

Hughes: Because of good communication in arbovirology, is there more collaboration in this field than in other fields of virology?

Reeves: I can't really answer that question, because I don't deal in other fields of virology that much. My impression is that there's some degree of collaboration in all fields, but in many fields of virology there's considerable work that isn't discussed before it's published, or territorial concerns are carefully protected. I think all we have to do is to look at the public press or the current scientific literature on AIDS to get examples of a lot of competition for credit, for glory, who does what first, and so on. We've had very little of that in arbovirology. We've had very little of people saying, "I did this first, and I want to get the credit and publicity out of it," and so on. Or saying, "I won't tell you what I'm doing, because if I do, you'll do it." In some other fields I know this happens. That doesn't mean there's no interchange. I think there's a lot more of individuality and a "me for me" attitude in other fields of microbiology and science generally than there has been in arbovirology. However, who knows? It may be changing now.

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<sup>1</sup>Lederberg, J., Shope, R. E. and Oaks, S. C., Emerging Infections Microbial Threats to Health in the United States. Institute of Medicine, Mate Academy Press, Washington, D.C., 1992, pp. 1-294.



Hughes: Why do you think that?

Reeves: I won't give you any examples of this. I just think that it's changing now, which I don't like, but I'm not surprised. People are people.

### Arboviruses and Disease

Hughes: Was and is arbovirus research limited to diseases of human or veterinary importance?

Reeves: By definition it almost has to be. There are plant viruses that are transmitted by arthropods, but we're not concerning ourselves with them. At this stage we have no evidence that any plant viruses affect man or other vertebrates, or that man and other vertebrate viruses affect plants. It wouldn't surprise me if we found in the future that it has happened, because after all, the arthropods we work with feed on plants very frequently as well as on vertebrates. Plants are a frequent source of sugar and a source of fluid for arthropods. Many plant and animal viruses look very similar under electron microscopy, which makes you think they may even smell the same or be related. You have the feeling that there almost has to be a relationship between them. Animals eat a lot of vegetation, and a lot of animal products go back into vegetation, but I don't have a good example of a virus that goes back and forth between plants and vertebrates. I think it's going to be found one of these days.

Hughes: Is every virus that is listed in the catalog associated with a disease?

Reeves: No. Only only a little over one-fifth of the five hundred and some viruses we know of have ever been associated with a disease. Many more than that have been associated with infection in people or in animals. As a matter of fact, by definition any virus in there has to have an association with an animal. There are beginning to be exceptions to this, in that some of the viruses are maintained by being passed by female or male arthropods to their progeny by transovarial or venereal transmission. It's conceivable that some of these viruses are maintained almost entirely as an arthropod virus and only get inoculated into vertebrates as an accident and produce an infection. It's also possible that these transovarial viruses periodically have to be given a boost by going through a vertebrate host, so that more arthropods get infected by feeding on infected blood. By definition all arboviruses have to have an animal host, and by

definition they're supposed to be dependent on arthropods for their maintenance except for the exceptions like hemorrhagic fever viruses. You might wonder if some of the viruses have escaped being dependent on an arthropod host, while the others depend primarily on arthropods for their survival.

The original question is still like the one, "Which came first--the hen or the egg?" Where do these things start? Do they start as arthropod viruses? Do they start as animal viruses? Now, you must remember that arthropods are animals; they are not plants or minerals. In this dialogue, by vertebrates we have been referring to birds and mammals. But we don't know the answer to the origin of these viruses. The minute we learn about transovarial or venereal transmission and maintenance of viruses in an insect or tick vector, we almost have to start to think of them as possible arthropod viruses. As a matter of fact, many viruses spend more time in insects or ticks than in the birds or animals. So again I repeat that we don't know the answer to the hen and the egg question.

Hughes: Is funding more of a problem when a virus cannot be definitively associated with a disease?

Reeves: Yes. The association with disease is almost essential to get any support. It is easier if it is a public health or veterinary health problem. You can work with viruses as a molecular biology problem, and they don't have to be associated with a disease. After all, Nobel Prizes have been awarded for research on viruses unrelated to any health problem. But it helps. The reviewers may say, "Why are you going to do this molecular biology?" You may say, "I'm doing it for science's sake," and some of that will be accepted. If it makes a better molecular model, they will say fine. But when you go out to shake the apple tree to get the money down for field biology studies and so on, the project had better be associated with an infection at least, if not a disease.

Hughes: What about funding for diseases that aren't a current problem in this country or without a direct link with American need?

Reeves: At this stage I would say it's getting very close to the point of impossible, and forget trying to do it. There are federal programs devoted specifically, even restricted to studies outside the United States. I feel that such funds are increasingly limited or are so restricted in how they can be used that it makes them of limited interest to people like me.

Hughes: Where does that leave a Third World country?

Reeves: I guess the general attitude is, "That's their problem." One of the things that I hope the National Academy of Sciences group will address, and I know the membership of the American Society of Tropical Medicine and Hygiene have, is: "The world is one world now. Any problem that occurs anyplace in the world is in part our problem." When you look at it realistically, such problems may come here, because our economy, general well-being, and happiness depend upon our assisting in the solution of health problems on a global basis. Right now I'm in a very discouraged state of mind as far as funding for any research in this field is concerned. I hope I am wrong.

Hughes: A good place to stop.



## VII CONSULTANT POSITIONS

### The Centers for Disease Control and Hemorrhagic Fever

[Interview 9: April 7, 1991]###

Hughes: Dr. Reeves, you talked earlier about your early connections with CDC. Maybe we should now move on to 1973 and hemorrhagic fever.

Reeves: Yes. These were somewhat different types of assignment with the Centers for Disease Control than I'd had before. The earlier appointments dealt in very minute detail with research that was very similar to what we were doing in California on mosquito-borne viruses and virus activity generally.

### The First CDC Meeting on Hemorrhagic Fever, 1973

Reeves: In 1973, CDC called a meeting in Atlanta, Georgia, to review a series of emerging diseases that had appeared in tropical areas of the world. It had started with Lhasa fever in Africa; there's a famous book called Fever<sup>1</sup> that was written on that. The Rockefeller Foundation was deeply involved in the original research, and then the Centers for Disease Control began studying it.

Then they had Marburg disease, which occurred in Germany. People who had been working with monkeys--autopsying them and

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<sup>1</sup>J. G. Fuller, Fever: The Hunt for a New Killer Virus. Readers Digest Press, New York, 1974. Distributed by E. P. Dutton & Co., Inc., 1975. Paperback, Ballantine Books, Random House, Inc., N.Y.

doing tissue culture with their organs--came down with this disease, and almost all the people who got the disease died. It was finally traced back to central and South Africa, where several people contracted it while traveling. Then Ebola virus emerged in Africa. But even before that, there was a virus disease that was occurring in Argentina, called Argentinian hemorrhagic fever, caused by Junin virus. That was a very serious disease in people who worked in agricultural areas. Then in the early 1970s still another hemorrhagic fever disease emerged in Bolivia, called Bolivian hemorrhagic fever caused by Machupo virus. Earlier another new disease had emerged in Korea during the Korean War. It was called Korean hemorrhagic fever, which is now known to be caused by Hantan virus. Originally this was believed to be a mite-borne disease, but it is now known to come directly from rats and mice to man.

Anyway, this whole series of diseases was emerging very rapidly in various parts of the world, and very little was known about the viruses that caused them. They were just beginning to find out that these diseases were all caused by rodent viruses, and they really didn't fall into the arbovirus category because they didn't involve an arthropod vector to transmit them. Rats or other small rodents that were infected in nature were the basic source of infection and shed the virus in their urine and feces, and that was the source of human infection.

But anyway, CDC called this meeting in 1973 and brought together various people from the Rockefeller Foundation, the Public Health Service, and the United Kingdom, who were working on these diseases. We spent the better part of three days discussing the viruses, what problems were unresolved in field studies, and what resources were available. It was a very interesting consultantship for me, because I wasn't at all involved with these viruses. I was there, I guess, just to make any comments that I felt were important. Another important thing that came out of this was that it established a very close contact for me with Karl Johnson, Tom Monath, and other people whom I subsequently worked with at other times on other problems. So that was interesting.

The CDC held another meeting in 1976 in Atlanta, which was the International Symposium on Arenaviral Infections of Public Health Importance. Again I was there as an observer to learn what was happening in the rapidly developing field of hemorrhagic fever.

**Dengue Fever in Puerto Rico, 1974**

Reeves: And then in the following year, 1974, I got a request to go to Puerto Rico, where CDC had a field station. There were two diseases they were working on. One of them was dengue fever, which had become a very important disease in Puerto Rico. In the 1960s, Dr. Scherer and I had said that dengue fever was a disease that had been eradicated from the Americas. Shortly after that time there was a series of epidemics in Puerto Rico, Cuba, Trinidad; a whole series of countries had outbreaks. Currently dengue fever is a very common disease all over Latin America and the Caribbean area, as well as in Asia.

CDC wanted me to go down and review what they were doing on dengue. At this stage we knew that this disease was caused by four different viruses which occur in the blood and cause a rash. A very discomfiting disease with a lot of aches and pains, it frequently was misdiagnosed as flu or something of that type. But it also has the complication of developing a hemorrhagic and a shock syndrome in some individuals. It can kill people, particularly children.

They had found that *Aedes aegypti*, which was the vector, was completely resistant to most insecticides, and they were finding that it was impossible to control the vector. They wanted me to review the program: confirm the sort of laboratory setup they should have, what they should be doing in their field studies, and things of that type. Incidentally, while I was there they also asked me to look at their program to control schistosomiasis, which is a very important and widespread worm infection in the tropics. It is a very interesting disease. It depends upon human excreta getting into water in order to produce the infectious stage, there is a multiplication cycle in snails, and then it becomes an infectious stage that can go through the skin of people and infect them. It infects the liver, kidney, and/or intestinal tract.

The trip was particularly interesting to me, because the person who was directing that program was Dr. Barnett Cline. A few years before that he completed his Ph.D. with me in Berkeley. He is a physician who has been involved for many years in tropical disease research. When he finished his Ph.D. with us here in epidemiology in 1973, he got the job with CDC to be in charge of the San Juan, Puerto Rico, Tropical Disease Laboratories. He became head of the tropical medicine parasitology academic program at the Tulane School of Public Health.



I also had an opportunity in Puerto Rico to collect orchids, which is one of my hobbies. I collected a fair number of orchids, which I was able to import because I had permits to do so. Some of the *Oncidia* are still growing in my house, and they're in bloom right now. They have long sprays of golden-colored flowers and are really spectacular at any time.

***Aedes albopictus* in Houston, Texas, March 12, 1986**

Hughes: Did you have further assignments with CDC?

Reeves: Yes, at this time I thought I'd probably done about all I could do for CDC except to continue to review the program in Colorado every several years. Then, unexpectedly, Dr. Bruce Francy called me from Fort Collins on March 19, 1986. He'd done a Ph.D. in epidemiology with me some years before. He was responsible for much of the activities of the Centers for Disease Control on interstate problems with vectors and vector-borne diseases. He sort of dropped a bombshell in my lap. He said, "We need you to come to Houston, Texas, three days from now." This was without any forewarning whatsoever. He said, "We have a very serious problem. *Aedes albopictus* has been found in Houston."

It was a bomb because as far as we knew, this mosquito was restricted to Asia and some of the Pacific Islands, including Hawaii, and was not known to occur in the continental United States. He was calling to tell me that it was well established in Houston, Texas. This is an area that we considered to be potentially receptive for introduction of dengue fever. There are other virus diseases, like St. Louis encephalitis, that are already carried there by mosquitoes.

To make a long story short, the CDC staff called together a very small group of us. The outside consultants were me; Dr. George Craig from Notre Dame, who has spent much of his life working with *Aedes* mosquitoes, particularly *Aedes aegypti* but also *Aedes albopictus*; Mr. George T. Carmichael from New Orleans, who was in charge of their *Aedes aegypti* control program there and had experience with mosquitoes that were very similar in habits; and Mr. Don Wometdorf, Chief of the Environmental Management Branch of the California Department of Health Services.

We sat down in Houston to discuss the significance of this finding with the representatives of the Harris County Mosquito Control District. Indeed, they had good evidence that this mosquito was well established in Houston. [tape interruption]

The entomologist for the district had recognized that this was the first time this mosquito had become established in the continental U.S.A. He had sat on the information; he hadn't written a scientific paper on it yet and didn't want the news to get out until he'd written his paper. I don't think he really realized how potentially important it was.

We went out in the field the first day and readily observed the mosquitoes in tire dumps. I said, "I want to go look in a cemetery." We looked in a cemetery, and there they were in the urns that hold flowers. These water containers are very attractive to this mosquito as a breeding site. I'd worked with this mosquito in Hawaii--where it's a common mosquito, but we don't consider that part of the continental United States--so I knew something about the mosquito.

The first question we asked them was, "How far has it spread from the immediate environment of Houston?" That's in Harris County, Texas. They didn't know. We said, "What you ought to do today is send inspectors out to each of the surrounding counties and have them look for this mosquito intensively, and call us back by noon tomorrow if they find it." By ten o'clock the next day the phone calls came in from all the surrounding counties that they had found *Aedes albopictus*, so we knew it had gone further out than just Houston.

We immediately were put in the position of being asked to make recommendations of what should be done. The first problem was to try to trace how far the mosquito had gotten, and the second question was how it had gotten there. It was quite obvious that it was widely associated with used tires. You can't imagine how common used tire dumps are in our environment. When a tire is worn out, it's indestructible and usually unusable, so they just accumulate in our environment. You try to bury them in the ground, and some way or another they work their way to the surface again; and you can't sink them in the ocean. They're impossible. You can't burn them because they make smog. They're big business, actually--what can we do with used tires?

So we recommended that CDC set up a review: where the mosquitoes had come from, how they got here, and so on, because Houston is a big shipping port. As a matter of fact, it's one of the most active, if not the most active, shipping port in the whole United States. Ships come in from all over the world to the gulf. We recommended that as soon as possible they should gather sufficient information to have a follow-up meeting on the problem. At that time we hoped it would be possible to make very widely important decisions on what to do about the problem.



The Second CDC Meeting on *Aedes albopictus*, January 15-16, 1987

Reeves: So on January 15 and 16 of 1987, CDC called a second meeting, this time in Atlanta, Georgia, and this was quite a large meeting. There were probably thirty people who attended the meeting. There were six official consultants,<sup>1</sup> each of whom was asked to write a report summarizing their recommendations. The first question was where this mosquito came from. *Aedes albopictus* is distributed from Japan in the north all the way down through Asia, including Thailand, Malay, and so on. It is a very common mosquito, sometimes called the Asian tiger mosquito. It's been in Hawaii for a number of years, where it's also an introduced mosquito. So we know there are different populations of this mosquito, and they extend all the way from tropical areas into temperate areas.

Well, fortunately, we did know that this mosquito was common in Japan, because during World War II they had epidemics of dengue fever there and had published papers on the fact that this mosquito had carried dengue fever in Japan. They didn't have *Aedes aegypti* there. People who were interested in pure mosquito biology had worked on *Aedes albopictus*, and they found out that there were markers on the genes of this mosquito, so they could identify populations from different geographical areas. They could do isozyme studies, which I won't go into detail about. They could differentiate the mosquitoes from the northern temperate regions from those from the southern tropical regions. The populations had been separated for some numbers of years.

The *Aedes albopictus* that occurred in Japan had adapted to surviving the winter there, so the eggs could live through freezing temperatures. When it was cold, the eggs just went into a diapause so that they weren't active, and they survived the winter. Then when the temperatures went up higher in the spring and summer and the water submerged them, they would hatch. The eggs of mosquitoes of the same species from the tropics were not resistant to freezing, so it made a very nice marker. We were told by Dr. Craig that the *Aedes albopictus* in Houston was the northern type, like in Japan.

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<sup>1</sup>Dr. Scott B. Halstead, Rockefeller Foundation; Dr. George B. Craig, Notre Dame University; Dr. Thomas H. Weller, Harvard University; Dr. Michael Osterholm, Minnesota State Health Dept.; Dr. Gregory R. Istre, Oklahoma Dept. of Health; and Dr. W. C. Reeves, Univ. of California.



They were able to trace the probable source of the introduction back to tire shipments from Japan. You won't believe it, but enormous shipments of used tires are made from Japan and other parts of Asia to the United States for retreading. The reason is that the steel-belted tires that you see on our vehicles can't be retreaded, but there's still a large supply of tires in Asia that can be retreaded. Particularly, this is true of the large tractor tires, airplane tires, and so on.

The other interesting thing is that Japan and Europe have laws against retreaded tires being used on buses and airplanes. If you fly on an airplane or ride in a bus here, every tire that you're riding on may be a retread. So there's a big market here, and tires from all over the world that are still good enough to be retreaded are shipped to the U.S.A. in huge containers on ships. They're packed in as tight they can be--an unbelievable number are put in--and then they seal up the containers and ship them off.

A lot of the tires have been sitting outside in Japan or in the South Pacific. They fill up with rain water, and *Aedes albopictus* and other mosquitoes go in there and lay their eggs. Even after the water evaporates, the eggs of *Aedes* are resistant to drying. Many of the tires get shipped wet, because if there's water in them, who cares; they travel in the containers. So when they arrive in the ports here, the eggs are all set to go or have already hatched. As a matter of fact, there are live mosquitoes in some containers. If there's water in there, there are eggs and larvae. There may even be adults flying around in the containers, but who cares? Nobody who is handling the containers cares. But now the U.S. cares, as we don't want mosquitoes to be introduced. There is always the chance that they could become established, could be carrying a disease like Japanese B encephalitis, or could become vectors of viruses that occur in North America.

So this posed an impossible problem, because we had no quarantine and had not set up regulations to control the shipment of these tires. This meant a whole new set of rules and regulations and laws had to be established. The real problem, then, was to get the retreading tire industry to be cooperative. Fortunately, they saw the handwriting on the wall: they'd better be cooperative or the shipments were going to be stopped. So the CDC learned how to treat the tires by fumigation and to require that they could not be shipped with water in them. We think the new regulations have really cut back, if not completely stopped, the importation of mosquito eggs. But it still is a tremendously big problem. *Aedes albopictus* wasn't the only mosquito that was found in these shipments when they started looking at them closely.

When news of the introduction of the mosquito became available to the news media, there was considerable excitement. They said, "This horrible Asian tiger mosquito called *Aedes albopictus* has become established in our country," and they gave it lots of publicity. I've got clippings that said, "We have to eradicate this mosquito from the United States because it's dangerous, it's going to transmit all sorts of diseases, it's going to pick up viruses that are already here, like St. Louis and LaCrosse viruses, as well as exotic viruses like dengue. Ergo we have to get the U.S. Congress to appropriate millions of dollars to eradicate the mosquito and do a wide variety of studies. That recommendation hit the CDC Atlanta, Georgia, and from there it hit Washington. It didn't go very far, because the question was, "What disease does it carry now in the U.S.A.?" We didn't have a disease associated with it here at this stage. There's been a big search, and so far one new virus (Potosi) has been found in them which doesn't seem to be of public health significance. Specimens infected with eastern encephalitis were collected in Florida in 1991 and with St. Louis encephalitis virus this year in St. Louis. This is causing increased concern.

So this mosquito has become firmly established in seventeen states within the United States. A tire was found in Oakland in a used-tire place, and it was full of these mosquitoes. That tire had come from Hawaii, and by the time it was known, it had been shipped back to Hawaii. This was interesting, as they couldn't get the tire and study it any further. Now, that obviously wasn't a Japanese mosquito, because the *Aedes albopictus* in Hawaii is the tropical variety that comes from Southeast Asia.

Simultaneous to this, reports came out of Rio de Janeiro and Brazil that *Aedes albopictus* was there, and that also was a new introduction. The immediate concern was that the United States was responsible for this, and we must have shipped it to Brazil. Fortunately it turned out that wasn't the case; the *Aedes albopictus* in Brazil was the variety from Southeast Asia, which is very well adapted to living in the tropics, and Rio de Janeiro basically is a tropical area. It seems quite probable that it has been involved in a recent dengue outbreak in Rio.

*Aedes albopictus* from the Southeast Asian area probably wouldn't establish itself in the northern parts of the United States because it couldn't survive the winter, but it would probably be able to survive in southern Texas, Louisiana, or Florida. So it was a very interesting assignment we had from CDC to review this new problem and then make recommendations on what studies needed to be done.



Some people who were on that committee still are very dissatisfied that eradication was not attempted. It would have cost millions of dollars to have done it. Our country was well known for the fact that we had not succeeded in eradicating *Aedes aegypti* when it had been eradicated from most of the western hemisphere. In the earlier years the United States wasn't doing hardly anything, and then they started an eradication program, which was very expensive. After three or four years it hadn't been completely successful, and in the interest of economy the U.S. Congress just killed the program. In the fifties and sixties the biggest program in the Centers for Disease Control, was the *Aedes aegypti* eradication program.

I found assignments of this type with CDC very interesting. It's sort of nice to be sitting here on edge and waiting for somebody to call you and say, "We have another problem, and we'd like to have your advice on it." I find the problems interesting, and I find them challenging.

Hughes: Do they call you whenever there's a problem concerned with mosquitoes?

Reeves: No, they don't call me anytime there's a problem with mosquitoes. They'd be calling me all the time every day. They have very competent people working in the CDC and in other parts of the federal government on mosquitoes and mosquito-borne diseases. They call in outside consultants when they want the opinion that comes out on the problem to carry some weight and to support a defensible program. It's not going to be just CDC's opinion. And indeed they may find out that the opinions they get from the consultants are not the ones that they want.

Member, Viral and Rickettsial Diseases Study Section of the  
National Institute of Infectious Diseases, U.S. Public Health  
Service, 1963-1965

Hughes: The next topic is NIH. Please start with the virus study section.

Reeves: I had quite a wide variety of appointments with the National Institutes of Health, usually with the National Institute of Allergy and Infectious Diseases, which is a part of NIH. My first experience with research grants was back in the early 1950s when they started their research grant program. We submitted applications and were funded for much of the research that we've done here in California, which I've already reviewed. So the National Institutes of Health has been a major financial supporter



of our research for many, many years. I think we may have the record for having over thirty years of continuous support from them on a single type of grant.

The first time I became involved in other ways with NIH was in 1963, when I was asked to be a member of the Viral and Rickettsial Diseases Study Section. The study sections in NIH are groups that they appoint each year from outside of the federal government. The members review research grant applications and assign a numerical ranking to them. Those rankings are done for the hundreds, even thousands of applications they now get on chronic diseases, infectious diseases, whatever it might be. The reviews done by the study sections determine the probability that a grant will be funded. Each grant receives a priority number, and there is a cut-off point of what percent can be funded.

It was a very interesting experience to be on the study section on virus diseases because, as I recall, I was the only person who had any knowledge of vector-borne viruses. The rest of the people were all concerned with influenza, hepatitis, infectious mononucleosis, poliomyelitis, or whatever disease you might want to mention, but I was the only one who represented this peculiar group of diseases that are caused by viruses that infect vectors, and have animals instead of man as their basic hosts, although a few, like dengue and yellow fever, have man as the host.

I also was one of the few people on the study section who had any experience at all in working in field epidemiology--that is, chasing mosquitoes and birds in the field and seeing where they'd been or whether they had antibodies. Most of the people on the study section were what I call bench research workers. In other words, they sit in a laboratory and work at a laboratory bench. They don't go out and collect specimens in the field, and they don't study vector biology or animal biology.

So one of my tasks on the study section was to educate the other members so that when an application came in where somebody wanted to study migratory birds going up and down the Mississippi River, it would receive proper attention. The bird project was going to cost a lot of money, as it entailed travel up and down the Mississippi flyway from New Orleans all the way to Chicago. It was necessary to mark and release these birds with bands on their legs and then see if they flew to Latin America or wherever. Maybe blood samples had to be taken for virus tests and so on. This project could be important to do. The purely laboratory-oriented person might say, "I can do an awful lot of research with that money. What's all this money for travel? Why money to rent a car? Why money to stay in motels?" So there was a real need to

educate people if you thought the research was important enough to be done.

Hughes: Do I remember correctly that Dr. Lennette served on the virus study section?

Reeves: Yes. He was not on the committee at the same time I was.<sup>1</sup> They're usually careful not to have people from the same organizations or the same geographical area on the committee at the same time, and they would know that Lennette and I were closely associated. The other thing is that when you're on one of the study sections, any time your research grant comes up for review they have a real problem, because it can't be reviewed by the group you are on, so they have to move it to some other review group. They may even have to appoint an ad hoc committee to review your grant instead of having it go to the committee that you're on. You certainly don't want to sit there and listen to them discuss it. The reviews get very bloody at times and very difficult.

Member, Study Section, Epidemiology and Biometry Research Training Program, U.S. Public Health Service, 1965-1967

Reeves: You can only be on a committee for several years, and then you have to be rotated off. They do that to make sure that they have representation from various places and fields. People aren't on them forever. So I went off of that section after a couple of years. I went on in '63 and came off in '65. At that time, to my amazement, they called me up and said, "We need you on another committee." You could be on a different committee as long as you weren't on the same committee. That was when I went on the Epidemiology and Biometry Training Grant Committee. This was completely separate from the research study sections, because this had to do with training in public health and related fields. They had training money that was given to institutions for student fellowships or to pay faculty support for people who were needed to train students and so on. This was interesting to me because we had an Epidemiology and Biometry Training Grant at the time that Dr. R. A. Stallones had been able to get established. So I went on that committee. I didn't find it nearly as interesting or as exciting as the virus study section, because at the virus study section meetings you always learned something new about viruses

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<sup>1</sup>Dr. Lennette served from 1951-1953 and was committee chairman 1952-1953. See his oral history for information on the study section.



and new technology that you didn't know anything about. When I went on the Epidemiology and Biometry Training Grant Committee in 1965, I found that all I was learning about was how many students they had and their educational program. None of it was very different from what I already knew about, so I didn't find it very exciting or challenging. I spent a year on that committee, not fighting the system but not being particularly excited.

I also made a couple of trips to Washington as an advisor to the Fogarty International Health Center of the USPHS. The first was in 1968 to serve on the Ad-Hoc Advisory Committee on the Implementation of a Professional Career Development Program in Global Community Health. The second was in 1973, which was a Planning committee on the role of the U.S. Biomedical Institutes in International Health Programs. The agenda for both of these meetings primarily focused on the importance of creating curricula and careers for young scientists in the international health arena.

Member and Chairman, Research Reference Reagent Grant Committee for Arboviruses, NIH, 1967-1972

Reeves: It was about this time that I had an opportunity to change my NIH appointment, and that is a story in itself. I had a telephone call from Bethesda from Dr. Karl Johnson. He asked if he could come to Berkeley to discuss a confidential matter. I didn't know Karl, but I knew who he was, so I said, "Sure, come on out." On arrival, he came in and said, "The NIH wants you to become the chairman of the Reference Reagent Committee for Arboviruses." He went on to say, "We need a graybeard; we need somebody who's old and has had a lot of experience." Well, I didn't like his opening remark too much because I didn't have a beard, number one. And number two, at that time I didn't have all gray hair; I even had some hair. Also, he had a beard that I didn't think looked too good on him. I decided at this time that he knew I wasn't too happy about his approach, and it was time for a break. I said, "Why don't we go and have lunch?" He gave a sigh of relief and said, "Fine." He also noticed the forty-pound striped bass I had mounted on the wall of the office. He said, "Do you like to fish?" And that revealed a mutual interest that kept conversation going through lunch.

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Hughes: How did you get on the committee after this miserable start?



Reeves: I let him know again that I hadn't appreciated his approach to me, but I still didn't even know the details of why they had selected me. I got him very worried; he thought I still might kick him out of my office. Anyway, I finally said, "Tell me what the problem is that you came here to talk to me about." He said, "I was just called up from Panama to be on this Research Reference Reagent Committee for Arboviruses. You know about that committee?" I said, "Yes, they're making reagents to be used for diagnosis of arboviruses and related viruses. It's a big program that the American Committee on Arthropod-borne Viruses sort of pushed down the NIH's throat, and the American Society of Tropical Medicine and Hygiene is behind it. I know that Wilbur Downs is the chairman of it, and I know the other people on the committee. What's the problem? You've got a good committee; it's a good program."

He says, "The problem is that when they started this committee several years ago, the people who were handling the appointment of the committee members forgot that people are only appointed for two years and then have to go off the committees. We just had this meeting, and Dorland Davis, the director of the National Institute of Allergy and Infectious Diseases, came in and handed everybody their little certificates, thanking them and saying, "You know this is your next to last meeting, because you all will have to be replaced because you've been on the committee for two years. The chairman has to be replaced, and almost everybody has to be replaced." To say the least, there was stunned silence, as this was news to the committee members.

Karl said, "I'm new on the committee. They just brought me in from Panama to be on the committee, and they said that I had to come out here and talk to you. They decided you're the person they want to be the next chairman of the committee, but they were pretty sure you'd say no." I said, "Well, they sure sent the wrong guy out here, because you got off on the wrong foot with me right away." He said, "I know, and I apologize. We really need you for this committee, and I'd like to work with you." I said, "It just happens, I'm already on the Epidemiology and Biometry Training Grant committee. I can't be on two committees." He said, "Would you be willing to drop that committee and get on this one?" I said, "Yes, I'd be interested in doing that. It sounds much more interesting, and I'd be working with friends, plus the fact that we have a bunch of new people to appoint."

So anyway, that was the beginning of a very interesting relationship with Karl Johnson, and it has continued for many years. I'll be mentioning it again as we get into some other consulting activities. Karl and I have become very close friends, not only science-wise but fishing-wise. He was also very much

involved in the CDC committee on the hemorrhagic fevers that I discussed earlier. As a matter of fact, he was a victim of the Bolivian hemorrhagic fever himself. He got it in Bolivia and was almost dead when they got him on a plane to Panama. His wife, who also was a physician, got very emotional and kissed him on his arrival; so she got it, and it almost killed her, too. But they both made it. A third person who was on that trip and got it was another one of my students, Ron McKenzie, who was in Bolivia working with Karl.

Hughes: Is there a vaccine?

Reeves: No vaccine, no immune serum, no drugs, nothing. These are the diseases you work on and become a hero. As a matter of fact, one of the army sergeants assigned there got it and died.

The Research Reference Reagent Grant Committee was a very interesting one to be on, and to replace Wilbur Downs as the chairman was also interesting. We approved contracts that resulted in a large group of reagents that were put into a national depository (The American Type Culture Collection), and they were distributed nationally and internationally to workers in the field.

Hughes: Is this in virology in general or just in arbovirology?

Reeves: That particular committee was just concerned with making arbovirus reagents, nothing but those, because at that time we knew we had hundreds of these viruses. The people at the Rockefeller Foundation, Jordi Casals in particular, had done a lot of the classification of these viruses, and it was obvious that laboratories could not function and identify these agents if they didn't have reagents that would specifically identify them. So it was a national and international problem, and the National Institutes of Health put enough money into making reagents to get that project off the ground. Some of the reagents are still being used today. We even broadened the bank to provide antisera to identify some of the hemorrhagic fever viruses and mosquito blood meals because it was something that nobody had and everybody in the field needed. We spent a couple of years on the project until we got the reagents all made and stored away. The basic job was done, so that program was finished. The reagents were still there, but no new ones were being made up; it was no longer an active group. In fact, I spent more than the usual two-year term closing out the program in 1972.



Member, National Advisory Allergy and Infectious Disease Council,  
NIH, 1973-1975

Reeves: I'd hardly finished up on that particular assignment when I received a phone call from Dorland Davis. He said, "Bill, this is Dorland. I want to give you another appointment, unless you feel you need a period free of such activities." I said, "What do you want me to go on now?" He said, "I want you to go on the National Advisory Allergy and Infectious Disease Council to the National Institutes of Allergy and Infectious Diseases." Now, the council is the top advisory group to the director of the institute, in the sense that they're advising on policy, they're reviewing what's going on with research grant applications, establishing priority areas where research is particularly needed, and putting extra research money into such areas. It's a very prestigious and important committee to be on, so I was very flattered to be invited to do it for a couple of years (1973-1975), and did so.

Middle America Research Unit, Panama

Reeves: It happened that the timing on this was not the very best, because at that particular time my son [William C. Reeves, Jr.] was working down in Panama with Karl Johnson at the Middle America Research Unit [MARU]. The NIH had just decided to close that laboratory. Everybody who worked there was being transferred someplace or was out of a job, and my son was one of them. So I'm being asked to take a new assignment in an organization that has for practical purposes just fired my son. I found that to be an interesting position. When I went to my first meeting of the council I said nothing to Dorland about it; I assumed that he knew the circumstances.

The first thing they did when I arrived was to take me into his office. I had known Dorland for years, and I knew his brother, Dave, very well, as he had worked in the Rockefeller Foundation program in Brazil. In the office there was a big American flag sitting in the corner, and they said, "You have to take the pledge of allegiance to the United States government." I said, "What? I can't believe this. You mean I do all this work for the army, I do all these appointments for the National Institutes of Health and CDC, and all of a sudden for the first time since I was in grammar school I have to take the pledge of allegiance? They said yes, so I did. Then we went into my first meeting of the council. There were some members of the council who were going off, and it happened that two of them had sons who worked in the Public Health Service. Dorland made a considerable buildup on how pleased they were to have both fathers and sons involved in his program. But then he made the bad mistake of



saying that he also was pleased to have me coming on the council, and that would continue this heritage. I said, "I'm accepting this appointment, but I don't want it to go in the record that my son works for you, as you just closed MARU and fired him." There was silence in the room, and I laughed; so everybody laughed, because they figured it was all right to laugh. In fact, I didn't have to worry about my son; he was going to do all right, as he was moving to the School of Public Health at Seattle. In addition, I still had a concern with the overall success of the NIH program and wanted to support it.

Hughes: What is your son's profession?

Reeves: He's a physician and works on viruses, but he hasn't worked much on arboviruses. He did some early work on arboviruses in Panama, such as Venezuelan equine encephalitis, dengue, and yellow fever. Actually, he has worked on a wide variety of viruses. He worked on the hepatitis viruses in Panama, where the Indians have a very high infection rate. When he returned to Panama in 1977 to work at the Gorgas Memorial Laboratory, he started studies on viruses and cancer. He became interested in papilloma and related viruses, and that is his current focus of interest at CDC in Atlanta, Georgia. He developed studies that took him--and still do--all through Latin America, projects on various factors associated with various cancers, particularly concentrating on papilloma viruses. Bill became the director of the Gorgas Memorial Laboratory but had to close it out over a year ago.

Hughes: Would you like to say something more about the history of the Middle America Research Unit?

Reeves: Yes, because that ties in with the National Institutes of Health. Shortly after Karl's and my first meetings associated with the reagent project, he became the director of the Middle America Research Laboratory. It was not part of the Gorgas Laboratory; it was a completely separate unit and outside of Panama in the Panama Canal Zone, a U.S. territory by treaty.

Karl had several young people there who were working on virus diseases that are transmitted by vectors. These were not the viruses that I work with, as they are transmitted by *Phlebotomus* flies, also called sand flies. They do not breed in water like mosquitoes; they breed in organic debris which is moist, in treeholes and places like that. They were carrying a vesicular stomatitis virus which was a fairly important disease in cattle in Panama. It was of interest because sand flies also were the vector for other viruses and protozoan diseases in that area.

So Karl had these two young fellows, Drs. Robert Tesh and Brian Chaniotis, who were working on sand flies. I had known Brian before because he was a student who took his doctoral degree in entomology here at Berkeley. They had found what looked like good evidence that the vesicular stomatitis virus was transmitted

by the female flies through their eggs to their progeny. It was a very difficult virus, because they couldn't readily infect *Phlebotomus* by feeding them on virus, but if the female was infected she could transmit it to her progeny. This potentially was the first real demonstration of transovarial transmission by a vector of any arbovirus. Work about that same time or shortly thereafter was done in Wisconsin on a mosquito-borne virus called LaCrosse virus, which is transovarially transmitted by mosquitoes. Bob Tesh and Chaniotis haven't really gotten the credit they deserve for what they did first in Panama.

Karl thought a visit from me would be helpful to them if I would review what they had found. So I spent the better part of a week down there going over in detail the work they were doing and pointing out to them that with the very low infection rates they were getting, transovarial transmission alone was not enough to explain the virus maintaining itself. It would just disappear from the population within a couple of generations. I don't need to get into more detail on that now. Karl likes to tell the story that they didn't want to believe my report, but he finally convinced them that they should nail a copy of the report over their desks and look at it every day until they understood it.

Anyway, it was a nice trip, and it was the occasion of my first extensive fishing trip with Karl Johnson. When we finished the review we went out in a small boat to the Perlas Islands, thirty or forty miles from Panama. We caught a lot of nice fish, mostly wahoo. We've been fishing companions ever since then, so our relationships are not only business but also an avocation. Fishing and virology seem to go together for me, because virology led to my fishing relationships with Wil Downs, Karl Johnson, Archie Hess, and other people in the field, almost completely independent from virus research.

I went to Panama several times to review different aspects of the Middle America Research Unit program until they closed the place. I only made one trip after my son went down there in 1970 and joined Karl, and later when Bill went to the Gorgas Laboratory in 1977. I said, "I don't think it's proper for me to do a review in a place where my son is working."

Hughes: Did you have anything to do with his appointment at either place?

Reeves: None; absolutely none. Bill applied and received the appointment completely on his own merits and independent from me.

Hughes: Why was the MARU unit closed?

Reeves: Well, NIH had a budget crunch. They had to save some money. They thought, "This place sitting down in Panama isn't nearly as important as our laboratories in Bethesda and our Rocky Mountain laboratory in Montana. We've got to cut budget, so why don't we just cut this thing out?"



Karl was off on a trip out of Panama in 1972, so he wasn't immediately told about this decision. He came back from his trip to find out that his laboratory was being abolished. He'd worked very hard, and he'd built up a very good group of young, very competent scientists. I'm a little prejudiced because my son was there, but I also mean the other people, like C. J. Peters and Bob Tesh--and I could name many others who were very competent and doing very good research.

Karl got angry about the laboratory closure. All of his young "Turks" wrote letters to their senators, and Karl let the directors of health programs throughout Latin America know what was being done. They were losing the services of MARU which had helped them on difficult problems. The question was what could be done to save the MARU. The result was that ambassadors for the U.S. all through Latin America were barraged with complaints. These complaints were fed to the state department and army departments in Washington. The secretaries of State and Defense called HEW and let off a blast about not being informed of the closure. The Secretary of State outranks the Secretary of Health, Education, and Welfare. The Secretary of Defense also called and complained, "You didn't tell me you were closing the MARU laboratory, and I have several officers assigned there." The wires had been pulled. The Secretary of State was wiped out. He knew nothing about the closure, and he called the director of NIH. It got pretty dicey for a while. The people in NIH weren't very happy about what was happening, and Karl wasn't happy; nobody was happy. There was a lot of smoke and quite a bit of fire.

When all was said and done, the NIH assigned a good portion of the budget from the Middle America Research Unit, the facility, and the research staff to the Gorgas Memorial Laboratory in 1973, but there would no longer be an official National Institutes of Health program in Panama. The Gorgas Laboratory had quite a transfusion when it got this money and staff. However, it also created a lot of problems, because the staff at the Middle America Research Unit was on the pay scale of the National Institutes of Health, and the staff at the Gorgas Memorial Laboratory was paid on the pay scale of Panama. So this created a lot of problems which had to be ironed out.

The merger took care of a problem which the Gorgas Laboratory had recognized back in the 1950s when MARU was established. I discussed this earlier when reviewing my associations with the Gorgas. There were two research units working practically on the same diseases and in some cases in the same area. They were really competitors at times. That problem was worked out.



Advisory Committee on Public Health Service Foreign Quarantine Activities, 1965-1966

Hughes: Then what was the next thing for USPHS?

Reeves: The Surgeon General of the USPHS decided to appoint a committee to review the activities and value of the foreign disease quarantine activities of the United States. The six classical diseases involved were yellow fever, plague, cholera, louse-borne relapsing fever, and typhus fever--the big killer diseases. Smallpox was the other one at that time, because smallpox hadn't been eradicated yet.

These diseases were included in international quarantine regulations agreed to between governments and the WHO and included requirements for vaccinations for international travel, restrictions on movements of people, and reporting of cases to WHO. In addition, the U.S. had its own restrictions on immigrants and movement of people as visitors to the U.S.A. The question was whether these restrictions were really necessary or of any value and whether the procedures we were going through were really protecting anyone from disease.

They appointed a very interesting committee.<sup>1</sup> Dr. John Weir was the chairman. It was a very good committee, and we went at the task methodically, starting in December 1965. We had five two-day meetings and finished it up in April 1966. The further we delved into the problem, the more we realized how much the program was costing and how little it was doing for the citizens of the U.S.A. In fact, the existing control program for infectious diseases in the U.S.A. made it an unreceptive area to most of the internationally quarantinable diseases. As an example, if we have good water supplies, cholera is not going to be a big problem. If we have a good detection-isolation system, smallpox is not going to be a killer disease and emasculate the population of the United States. An epidemic of relapsing fever or typhus fever carried by lice could be controlled, and we hadn't had a case reported in the United States for fifty years. Plague was already endemic in the

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<sup>1</sup>Dr. John M. Weir, Chairman, Director of Medical and Natural Sciences, the Rockefeller Foundation; Dr. Carl A. Brandley, Dean, College of Veterinary Medicine, Univ. of Illinois; Dr. Howard B. Calderwood, Officer in Charge, International Health, Dept. of State; Dr. Geoffrey Edsall, Prof. of Microbiology, Mass. Dept. of Public Health; Dr. John C. Hume, Assoc. Dean, The School of Public Health, Johns Hopkins Univ.; Dr. Rodney C. Jung, Prof. Tropical Med., Tulane Univ. and Director of Health, City of New Orleans; Dr. Mack I. Shanholz, Commissioner of Health, Commonwealth of Virginia; Dr. Abel Wolman, Emeritus Prof. Sanitary Engineering, The Johns Hopkins University; Dr. Robert I. Hood, Chief Medical Officer, International Quarantine WHO; Dr. William C. Reeves, Prof. Epidemiology, Univ. of California.

western United States. Yellow fever was believed to be a low risk in the continental U.S.

Then we found out, as we dug into this matter, that a lot of these regulations were being extended by federal or state agencies to cover other problems. To give you an example, if people who lived in Mexico had a job in Texas, they had to pass a physical examination to make sure they didn't have any venereal disease or lice, and if any of these things were found, they couldn't come in. They had to have an X-ray for tuberculosis. If they'd forgotten their X-rays, they were X-rayed again. We found one poor guy they X-rayed maybe twenty times in a short period, and we didn't think that was the best thing for his health. As we got into the details, and I confess I was guilty of this one, I said, "I'd like to know what the relative risk is of a person getting venereal disease or tuberculosis by staying in Mexico or going to Texas. It turned out that if the guy stayed in Mexico, he probably wouldn't have as much of a problem as he would in some areas of Texas. The Texas State Health Department director didn't like that very much.

I never will forget, one day John Weir came into a meeting and said, "I just came in from Africa a short time ago, and I'll admit I had too much to drink on the plane, and was awfully tired. I was stumbling around. I'd been in an area where yellow fever, cholera, malaria, and a variety of diseases were present, and my yellow fever vaccine certificate wasn't up to date. When I got off the plane, the public health inspector at the bottom of the stairs, who was looking for sick people, said, "Dr. Weir, it's nice to have you back to New York. Welcome home." Weir said, "I shouldn't have been allowed into town. I should have been isolated and revaccinated, which wouldn't have helped if I was infected. I just don't think the system's working."

To make a long story short, we decided that the regulations and all the inconveniences for travelers were not of much importance. The report was submitted in June 1966.<sup>1</sup>

Hughes: Were the quarantines abolished?

Reeves: There never really was an effective quarantine in force. Of course, this was the other weak thing. You see, quarantine means you're going to be isolated for a certain number of days. That means if you get off that plane and are suspected of having a quarantinable disease, they could put you in the hoosegow; they would lock you up, and you would stay there through the incubation period, whether it's fourteen days, twenty-one days, or whatever it might be. But basically, what were they doing? If you came in to New York or to San Francisco from a yellow fever or smallpox

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<sup>1</sup>Report to the Surgeon General of the Public Health Service by the Advisory Committee on Foreign Quarantine, pp. 1-69, June, 1966.



area and you didn't have a valid vaccination card for the disease in question, they vaccinated you; but that didn't prevent you from being a source if you already were infected. It didn't cure the infection. The alternative was to give the traveler a card that said, "You have come from such and such an area where such and such a disease is occurring. If you become ill in such and such a period of time, see your physician and give him this card." The inference was that your doctor would know what to do. You know where we found those cards? In the circular file, right outside the airport door. Most of the cards were in the wastebasket. Nobody worried about it except the janitor. So we decided it wasn't doing much good, when a poor guy showed up from Mexico and had some lice in his hair. They took him in and got rid of his lice. If he had a venereal disease, they'd send him home. They didn't treat him. Most of these things didn't seem to us to have much relevance, so we wrote a report that said so and figured, well, that took care of that, because nothing will ever be done with regards to all these WHO agreements with the governments of the world.

By golly, the next thing we knew, President Johnson had a meeting with the president of Mexico, and he wanted to do something to make things go well. He said, "You know, we're going to stop all this quarantine nonsense between our countries." The Mexicans were beginning to put the screws on American tourists coming down; they were looking at their hair and so on and deciding they were going to get even with the U.S. inspectors.

Anyway, so big politics were made out of this, an agreement was reached between Mexico and the United States, and the problem disappeared.

Hughes: What about the international regulations?

Reeves: The interesting thing is that within a few years almost all of the world had accepted our findings as also being applicable to their situation. There still are some countries that haven't accepted the changes. There are still countries that require cholera vaccination even though it's not going to do any good. Even after smallpox had been declared eradicated from the world by WHO, some countries still required a smallpox vaccination for entry. Complications from the smallpox vaccination were a heck of a lot more dangerous than the disease, because there wasn't any smallpox left.

So I found this a very interesting and very educational experience. Everyone on the committee was an expert on some critical and pertinent area. I was the entomologist, and four of the six quarantinable diseases were carried by insects.



Attempts to Control Filariasis, Tahiti, 1970-1973

Hughes: Before we leave NIH, should we talk about the Tahiti visits?

Reeves: Oh, yes. That was a great experience. Dr. Leon Rosen, who is one of my former students, took a master's degree here in 1949-1950. He had also been one of our first medical students who had come to Bakersfield for training in 1946-47.

He had a project in French Polynesia that he thought I'd be interested in. When he'd done earlier work on filariasis, he'd determined that the *Aedes polynesiensis* mosquito was the primary vector of filariasis down there, and later he'd done studies on epidemics of dengue fever that occurred in Tahiti and had shown that *Aedes polynesiensis* also was the vector of dengue fever. So they had two diseases that were important to public health in Tahiti. *Aedes polynesiensis* was carrying filariasis, which gave chronically infected people enlarged legs, breasts, and scrotum. They couldn't work effectively because it plugged up their lymph glands, so they'd accumulate fluids in their extremities. Then dengue fever would come through, and this also could be a very serious disease. They had tried every way they could to control *Aedes polynesiensis*. It's a mosquito that has many of the same characteristics as *Aedes aegypti* and *Aedes albopictus*, and it breeds very readily in artificial containers around houses--vases, Coke bottles, beer cans, and so on. But it also breeds in rain water that accumulates in coconut husks. If you want to see natural containers, go where the coconuts grow and they're harvesting copra, and you see nothing but big piles of empty coconut husks, as they split the coconuts in two, which gives you two nice coconut shells. The mosquito lays its eggs in the shells, and when it rains the mosquito eggs hatch and produce more mosquitoes which can transmit filariasis and dengue fever. It's a wicked cycle, as the people are dependent on coconuts, and at the same time a byproduct is disease, which not only is a concern to the people but can decrease the tourist trade, which is an important economic resource.

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Reeves: Leon was also in touch with Dr. Lloyd E. Rozeboom at Johns Hopkins, who had been his professor when he did his doctor of public health degree. Leon has really had extensive training. He has a medical degree from San Francisco Medical Center, a summer training program with us in Kern County, an MPH in epidemiology from Berkeley, and a Dr. P.H. from Johns Hopkins University. He showed poor judgment about the site for the last degree--he should have done it at Berkeley--but that's beside the point.

But anyway, Dr. Rozeboom became interested in *Aedes polynesiensis* and had a colony of it. He also was interested in *Aedes albopictus* and had gotten a colony from Leon from Hawaii. He found that when he put these two species together in a cage, the males of the *Aedes albopictus* were extremely aggressive. They not only mated with the females of *Aedes albopictus* but they also mated with all the *polynesiensis*. They were more aggressive than the *Aedes polynesiensis* males, who politely stood aside and let their brothers go to work. When the *Aedes albopictus* bred with *polynesiensis* females and inseminated them, the females were sterile, and the females only mate once. So this looked like a beautiful new approach for biological control of *polynesiensis*. It was done in small cages first and then in rooms the size of this one that were maybe twenty by fifteen and ten feet high. When the two populations were put together, pretty soon there were no more *Aedes polynesiensis*; only *Aedes albopictus* remained.

Leon was intrigued with this, because *Aedes albopictus* couldn't carry filariasis. It was an incompetent vector, and even though it feeds on man it simply didn't transmit filariasis effectively. He knew it also could transmit dengue, but so what? *Polynesiensis* transmitted both filariasis and dengue, but *albopictus* only the one disease. So he got the bright idea, let's go out in the Pacific and introduce *Aedes albopictus* where it isn't, and as they will replace *Aedes polynesiensis*, this might control filariasis.

Leon had done a lot of work in Tahiti, because he'd worked there for several years on filariasis, and he'd done studies on measles and dengue epidemics. He'd done all this work for them. He said, "Let me introduce this mosquito into Tahiti and see if it will reduce and eliminate your native vector of filariasis."

Well, you can imagine the response. They started worrying about it: "No, you can't do that. Then we'd have a real problem. We'd have twice as much dengue." He said, "No, it won't work that way. You probably won't have twice as much dengue, but you'll get rid of filariasis." To make a long story short the answer was "No, no, you're not going to introduce that mosquito here." Leon's a very stubborn sort of a guy, and he said, "There's a lot of *polynesiensis* here. What if we went to one of the outer islands and did it?" "No, no, there are still people there. You can't do that."

William A. Robinson had been one of the people who had been an instigator for the filariasis research by USC [University of Southern California] that Leon Rosen had been involved in. He was a wealthy and influential man in Tahiti. He said, "Look, I have



this small atoll called Taiaro that I bought after World War II. It's three hundred miles from Papeete. Two people live there who work for me. Nobody else is allowed on the island. Why don't you do the experiment out there? I've built a house out there, and I'd be happy to have you use the house." He also had provided the house in Tahiti that they had used as a laboratory for their filariasis work way back in the forties.

So Leon went to the officials. They finally agreed that Leon and Duane Gubler could do the experiment on Taiaro, this little island that was just three miles across and nine miles around. Nobody went there except once or twice a year to pick up the copra, and the two people who lived there harvested it. Needless to say, there was no filiarisis or dengue on Tairaro. Of course we found out later tht the island was not too far from the French atomic bomb test area, so maybe they figured that if something went wrong on Leon's project they'd just drop a little atomic bomb in the middle of the atoll and do a bomb test while abolishing the mosquitoes. It would have worked, too.

Leon asked me if I would like to go along as a consultant to the project in 1972 and 1973. He was right; I wanted to go to French Polynesia, and I said yes, that would be fine. So I made two trips out there with him and others. To make a long story short, it was the driest period for rain they'd had in ten or twenty years. *Albopictus* stayed there for a couple of years, but it never replaced *polynesiensis*, and then *albopictus* disappeared. Meanwhile, we were getting nothing but flak back in the United States about the horrible experiment. We are still accused by a few people of doing bad science. But anyway, it was an interesting trip with an interesting outcome.

Just to finish up this story, one night we were sitting on Tairaro after working all day from sunup until dark. We had a little generator making enough light so we could see through our microscopes to identify mosquitoes. Leon is on his shortwave radio talking all over the world. You didn't have an awful lot you could do except work. If you didn't take something with you, you didn't have it, as you couldn't go down to the corner drugstore to buy something. You were just stuck there with your friends.

We were coming to the end of the project in January 1973, and we now knew that *polynesiensis* was still there and *Aedes albopictus* hadn't established itself. Leon said in a very worried voice, "Do you suppose that with these negative data they'll still publish this paper?" I said, "Yes, I think so." He said, "How can you be so sure? You're always too sure of things." I said, "Leon, are you president of any society today?" He said, "Yes,



I'm president of the American Society of Tropical Medicine and Hygiene." I said, "Is there anyone else on this island who's ever been a president of that society?" He said, "Well, you have been, haven't you?" I said, "Who was president of the society after me? It was Rozie [Rozeboom]." So three presidents of the society are sitting there, and he's worried. I said, "Do you think that Dr. Paul Beaver," who was the editor of the journal, "is likely to turn down an article by three presidents of the society? I think it might be published even if it's a terrible paper." Leon said, "I don't know. You get some awful tough reviews sometimes." I said, "You asked me. I think it will be published," and it was.<sup>1</sup>

It was an interesting experiment, and it was done in a unique environment. I'd never been in an environment anyplace else, and I still haven't, where if you didn't want to talk to somebody, you didn't have to. If you got a deck of cards out and dealt out a hand of solitaire, nobody was going to look over your shoulder and tell you what you had done wrong. It was a unique experience, especially when the boat didn't come back to pick us up at the time it was supposed to pick us up.

Hughes: Then you got worried?

Reeves: Well, we had a radio, fortunately. It was three hundred miles by a little inter-island boat from Papeete, Tahiti, out to Taiaaro. That was a long way in a little inter-island boat. There was no shower or bathroom on that boat, and the guy who as doing the cooking was a native with a dirty gray apron wrapped around his waist. He served up the food, and you ate or you didn't eat at all. Fortunately, he was cooking everything.

The first time I went to get my food, he took this glass bowl down, held it up, and looked at it. It looked a little dirty, so he breathed on it and wiped it off on his dirty apron. [laughter] This guy looked to me like he sure in hell had active tuberculosis. After that I slept with my own bowl, and I washed my own bowl. He thought that was very nice of me to wash my own bowl; but to sleep with it, he didn't understand. I wasn't taking any unnecessary chances.

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<sup>1</sup>L. Rosen, L. E. Rozenboom, W. C. Reeves, J. Saugrain, D. J. Gubler. A field trial of competitive displacement of *Aedes polynesiensis* by *Aedes albopictus* on a Pacific atoll. Am. J. Trop. Med. Hyg. 25: 906-913. 1976.

Commission on Viral and Rickettsial Diseases, Armed Forces Epidemiological Board, 1945-1970, and other Consultant Work with the Army

Hughes: Now, the army?

Reeves: Yes, we can go to the army. I've talked a lot about the army already in the sense of the trips that I made to Okinawa and Guam, and visits to the various laboratories to review their research programs and appointments to committees. Those were all very good experiences. Then I had various appointments as an expert consultant in entomology to the Surgeon General that went on for many years. Have we reviewed the army commissions at all?

Hughes: We've reviewed some of this earlier.

Reeves: To make this review complete, the Commission on Viral and Rickettsial Diseases was a part of the Armed Forces Epidemiological Board activities and was a primary source of funding for some of our research. It also provided contact with the leaders in many of the fields of virology. I was an associate member of the commission from 1945-1959 and a full member from 1959-1973, when they abolished the Armed Forces Epidemiological Board and its commissions. The Army Research and Development Command still had a lot of contract work that was being done by civilian organizations, and they still had to have a review system from outside of the armed forces to assist them in making decisions on priorities for those activities.

I was asked in 1973 to be a member of the Ad Hoc Review Group on Viral Diseases for the Army Research and Development Command and chaired it from 1981-1983. Later I was asked to chair an Ad Hoc Review Group on Medical Entomology from 1981-1982. They called these ad hoc committees. They came under the Armed Forces Research and Development Command, which the general responsible for such research heads up. The research included development of drugs, treatment of wounds, accident prevention, and control of infectious diseases. At that time they had relatively large contracts on hepatitis, enteric diseases, malaria, and the various arthropod-borne viruses. They were just establishing a system to carry out these reviews, and they wanted to make sure that the reviews were going to be as stringent as those done by NIH study sections, because they didn't want to be supporting second-rate research.

We very quickly discovered that there were some problems, because some of us also were serving on NIH study sections and councils. We knew who was in the business and who was getting



money for research from the National Institutes of Health. We found, to our amazement, that some persons were asking for money from the army and the NIH to do the same thing. Such projects had to interdigitate so there would be no duplication of funding from the federal government for a person to do the same research. It really amounted not only to doing rigid reviews of whether it was good research, but also making sure that there wasn't any double-dipping. The final arrangement was that we had an NIH representative sitting in on our reviews to assure us this was not happening, and all such support was carefully checked by both agencies.

The army also wanted to make sure that it was getting the best people it could to do their research in the various areas, so it was a very busy sort of consultation, and it was very educational to be able to recommend persons to serve on these committees. When I chaired these committees, I was always asked for advice on appointments. You find that if you can do this you get excellent committees. The army also wanted to have people from the National Institutes of Health and a wide range of universities on the committees.

I also found that when I went to Walter Reed Army Institute of Research for research review meetings, I was asked to give lectures in their tropical medicine and preventive medicine courses. That was fine, because I got to meet many of the young officers who were coming into the service who later became leaders in research, and some would later be assigned to us at Berkeley for advanced academic training in epidemiology and virology.

Hughes: Is there any general distinction between military and civilian personnel in the armed forces research programs?

Reeves: I guess I would say no at this point in time, except that civilians are more likely to be left in an assignment at one site, while military are frequently moved. There was a time when relatively little research was being done in the army. I would say that the big increase in research by the army, like at the Walter Reed Army Institute of Research, was during and post-World War II. I did a review of the medical entomology programs within the continental U.S.A in the early 1950s. I visited all such programs that existed at that time. The Walter Reed Institute program amounted to two people assigned there in entomology. For practical purposes, no research in virology was being done at that time; a little was being done on influenza and some of the rickettsial diseases.

Armed forces research programs in microbiology, parasitology, and medical entomology really boomed post World War II. I think



it was because infectious diseases were so important during World War II. There were more casualties from disease than from bullets and bombs. Research was the only answer to many problems.

During World War II, the navy had developed a series of research groups called Navy Medical Research Units. These were numbered in sequence. NAMRU I was organized by Dr. A. Krueger at Berkeley. He was professor of bacteriology and commissioned a series of scientists to work primarily on respiratory diseases. NAMRU II was developed by Dr. Thomas Rivers of the Rockefeller Foundation, and it was stationed on Guam. After World War II, it moved to the Phillipines. NAMRU III was developed in Cairo, and there was a unit at Great Lakes. The army developed a large microbiological unit at Fort Detrick, Maryland, and another at Dugway, Utah. At the same time, they developed the Armed Forces Epidemiological Board and its commissions to assure full use of civilian capabilities. Many of these research organizations and contract activities still continue today.

When I rotated off being the chairman of the ad hoc group to review research contracts on viral diseases, I thought I was through. However, a year later I was asked to become the chairman of the ad hoc committee to review contracts in medical entomology. Again it was a time when the majority of committee members had finished their two-year stints, so I had an opportunity to participate in selection of new members.

So for several years we reviewed all the research contracts the army had on various aspects of medical entomology. In one of the economy binges they decided they were going to have to cut out some of these committees. So they split up the activity of the entomology committee and put some members on the viral and some on the parasitology diseases. They disbanded the medical entomology committee in 1987.

Next week I am going to Frederick, Maryland, for a meeting of the U.S. Army Medical Research and Development Advisory Committee, invited by Commanding Officer Major General Richard Travis of the Army Research and Development Command, to review their present program. I have not received the agenda yet, but I am certain we will be briefed on research needs that have evolved during the "Desert Storm" war and experiences in the Arabian Gulf theater. It will be interesting to learn what the unanticipated disease-related problems were.

The interesting thing about these meetings is that we don't sit there and go through a lot of classified material. Rather we review present and anticipated health problems encountered by the armed forces, many of which are unique to the geographical area or

circumstances where these people find themselves. We are then briefed by scientists on the research programs being carried out to resolve those problems and are asked to comment on the adequacy of that effort, the need for expansion, or alternative approaches.

The problems faced by the military can be very difficult, as troops may have to be moved to tropical areas, desert, or the arctic, where unique diseases occur that have never been experienced by a person in the U.S.A. They have had no prior experience with these exotic infections, have no immunity, and can quickly become casualties. It is essential to develop a vaccine, drug, or insect repellent to protect these troops. The current major push is to develop a vaccine for malaria, as the malaria parasites in many areas of the tropics have developed resistance to most available drugs. It is a never-ending task to anticipate and develop effective means to protect troops or civilians in these circumstances. The same thing is true when new diseases emerge, such as the hemorrhagic fevers in Africa, South America, or Asia.

In May 1988 I was asked by General P. K. Russell to develop another committee to review all of the medical entomology research that was being carried out by units that came under the Armed Forces Research and Development Command. This would entail research by army and navy units within the U.S.A. and overseas. It would require reviewers to be competent in the range of infectious diseases carried by arthropods and utilization of the most recently-developed laboratory techniques and methods for control of a wide range of arthropod vectors. I was fortunate that we were able to get what I considered a small but "blue ribbon" committee<sup>1</sup> to work with.

We reviewed all the programs by visiting the facilities in the U.S.A. and by bringing key researchers to report to us from facilities in Egypt, Malaya, Thailand, Korea, Latin America, and the Philippines. Each unit provided us with detailed progress reports and plans for future research. Our committee developed a very detailed report,<sup>2</sup> and we are still getting reactions to that report. I believe it had a major effect and strengthened many programs.

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<sup>1</sup>Dr. L. H. Miller, National Institutes of Health, Bethesda, Md.;  
Dr. B. F. Eldridge, Department of Entomology, Univ. of California, Davis;  
Dr. J. H. Oliver, Department of Biology, Georgia Southern Colleges;  
Dr. J. D. Edman, Department of Entomology, Univ. of Massachusetts.

<sup>2</sup>Review of the Medical Entomology Research Program within the United States Army Medical Research and Development Command. March 10, 1989.



I am not infrequently asked on my return to Berkeley from one of these visits, "Well, what classified secrets did you learn about on this trip?" It's not a very smart question, really, and is easy to answer, as I can say, "None." In fact, there is a general belief that good research in the armed forces as elsewhere should be published in the best journals, and it is. On the other hand, if we were in a war or other tough situation, I wouldn't hesitate at all to be involved in dealing with classified information, because I think sometimes secrecy is absolutely necessary. In today's world it rarely is important.

Hughes: When I interviewed Dr. Russell,<sup>1</sup> he told me over the phone that you know how to run a committee and how to get a lot of work out of a committee. What is your process?

Reeves: It's flattering that Phil would say that, but I certainly have no unusual process. If I had to make a guesstimate of what works, it is that I think everybody on a committee has to have a feeling that I'm trying to give them a chance to express their opinions and am listening to them. I think that is important. Also, if possible, it is important to have a consensus; but if it is impossible to have consensus, you still must reach decisions. Another thing is that the chairman must know the agenda and have read all the background material beforehand. The chairman has no choice; he must have done this or he can't preside and stay on schedule.

When we did the review of the medical entomology program, we had a serious problem when some of our working material, such as project reports and plans, did not get to us until we were at the meeting. To say the least, that was extremely difficult. All I could say as chairman was, "We cannot review your program if you hand it to us here and then ask us to review it. We're not going to do it now; we're going to delay the review until tomorrow, and we'll expect you to be here to defend it. We have not had a chance to study this documentation, ergo we're not going to review it now, and it isn't the committee's fault." What else can a chairman do? You don't know what the hell they have done or want to do. As a chairman or committee member, I take these assignments seriously and review them because I want to be sure the armed forces have good research programs, adequate support, and that they get the job done. At times you have to advise them that major changes must be made or that a program should not be continued.

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<sup>1</sup>Telephone interview with Dr. Philip K. Russell, April 1, 1991.



Hughes: Did this technique that you developed always succeed?

Reeves: I'm sure it didn't. You asked me to talk about it, not to evaluate its success, and I have. As far as I can tell, if it is working, what I said must be why it's working. I don't know of any secrets to success in running a committee. Sometimes everybody thinks I'm nasty and/or difficult, but I'm not really. I'm a pussycat. No, I don't know any secrets to running committees except to stay awake. Sometimes I wonder if success is because I'm so old the committee members don't want to hurt my feelings, so they work very hard on their homework and write good reports. It's nice that Phil Russell said I do a good job. Two-star generals don't say that to me very often.

Hughes: He's very complimentary in general about you.

Reeves: Not necessarily to my face, but that was a good pun you just made about the general.

Member of the Secretariat, World Health Organization Expert Panel on Virus Diseases, 1960-1991

Hughes: I guess we're up to your work with the World Health Organization.

Reeves: My experiences with WHO have been very interesting but have not been too frequent. As we said earlier, the American Committee on Arthropod-borne Viruses started stressing the importance of arthropod-borne viruses and did it with assistance from the WHO. The Rockefeller Foundation organized the meetings in Lisbon, where we had scientists from many countries get together to talk about the importance of arboviruses and unsolved problems, and then the WHO endorsed the main points that came out of that meeting. It was obvious that WHO was going to try to put something on their agenda about arthropod-borne viruses. Then in 1966 they had their first group, their so-called Study Group on Arthropod-Borne Viruses. I participated in this and I felt very honored to be asked to join in that meeting. However, it was explained to me that I couldn't be a member of the committee. I said, "Then how am I going to participate?" They said, "You're going to be part of the secretariat." Secretariat means you will be in the group that's going to do all the dirty work. They like to say they have selected a chairman or co-chairs who preside at a meeting and determine its success. Well, the chair does run the meeting; but the members of the secretariat have to take minutes on the whole damn thing and work late at night writing up a report. So my

advice is avoid being in the secretariat group for WHO unless you really want to work.

They explained that the trouble is, "We have a limit on how many people we can have from each country on a committee. Dr. R. M. Taylor is going to be the chairman of this committee, and Prof. D. Blaskovic from Czechoslovakia is co-chair.<sup>1</sup> We can only have limited representation from the United States on this committee. We have a Russian, we have a Latin American, we have someone from England, and so on and so forth. But we want you to come, and Dr. Taylor wants you to be there to be part of the secretariat." So I said, "Okay, fine. This will be an interesting experience."

I was told to be there several days before the meeting. I learned that several members of the committee were supposed to have written position papers and hadn't. So on my arrival I was told, "You have to write these two working papers so we will have official documents for the committee to consider." I said, "Well, do you have this book? Do you have that book?" They said, "We'll have to go to the library and see if we have them." It turned out that they had some of the books, and others they didn't have. I wrote a paper on vaccine development that became the working document. My knowledge was very limited, and fortunately everyone's was. Now, this is like preparing a working document that can become the Bible on a topic, because the working document is the background the committee has to work with. Then they develop their report, and that is the Bible internationally. At least that is the way many people in the world look at one of WHO's reports or working documents. It's the Bible; if the committee accepts it, the world's experts have approved it.

I was also asked to write a paper on mosquito vector distribution and movement for the world. Again, I'd done a flight range study on *Culex tarsalis* in Kern County, but I certainly didn't have an encyclopedic knowledge of what the distribution of mosquito vectors was in the world or details of their movement. Again, the literature that I knew I wanted to prepare for this document, they didn't have. To be frank about it, I wrote the report off the top of my head; in other words, I winged it.

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<sup>1</sup>Members of the committee were: Major E. L. Buescher, Wash., D.C.; Dr. J. D. Gillett, Entebbe, Uganda; Dr. Hernando Groot, Bogotá, Colombia; Dr. J. A. R. Miles, Dunedin, New Zealand; Prof. A. K. Schubladze, Moscow, U.S.S.R.; Dr. C. E. Gordon Smith, London, England; Dr. R. M. Taylor, Berkeley, Calif.; Dr. H. Trapido, Poona, India; Prof. D. Blaskovic, Bratislava, Czechoslovakia.



Then I ran into horrible problems. The WHO secretaries were all from England, so they spent all their time correcting my spelling, my English, and everything else, so by the time I got the report back it didn't read like anything I had written. The vocabularies are amazingly different, even in scientific writing. The British, the Australians, and the Americans have differences in spelling, and those ladies were stubborn. I edited it all back into American, and they put it all back into "English" English. I finally gave up. It was like getting into a bragging contest, where you go first, and the other guy can always top you. It wasn't worth the effort. My American friends read my reports and thought I had been in Geneva too long.

Then many people came to the meeting with their own agendas. I won't even refer to names or to countries, but one of the people on our committee had been sent by a country to sell this particular tissue culture method to grow viruses. It was a terrible tissue culture system. We knew about it, and we knew it was no good. That person came to the meeting with relatively little scientific knowledge, sent there to sell that particular tissue culture as an agenda item, and it really had nothing to do with the purposes of the WHO meeting.

Hughes: How did the committee handle that?

Reeves: They just let the person talk and get it on the table. It never got through to the final report. It became that person's problem on returning. The person who was head of virology for WHO, Dr. Arturo Saenz, actually organized this meeting. At an earlier time he had worked with the Rockefeller Foundation group on arboviruses in Latin America, so he was realistic. He and the committee were very productive about what the problems were and what ought to be done. The difficulty was that each day when the meeting was over, the majority of the committee had no intention of doing any more work that day, and by the next day they expected to have drafts of reports and the proceedings.

As a member of the secretariat, my job in large part continued to be to spend a large part of each night working on these summaries, so I came in with toothpicks under my eyelids the next day. C. E. Gordon Smith was a member of the committee. Gordon is from England, just now retired as the dean of the London School of Tropical Medicine. I don't know whether he had insomnia or took pity on me, but he was very happy to spend all night working with me. He was a member of the basic committee, not a member of the secretariat. There were several other members of the group who were quite willing to work very long hours and to participate until everything was done. Gordon and I became lifelong friends. When the formal meeting was over, Dr. Telford



Work (Rockefeller Foundation), who also was a member of the secretariat, and I stayed afterwards for a week to transform the findings into a working document.

Hughes: Do you think members of every WHO group do things this way?

Reeves: I think so pretty much. You have to realize that WHO, generally speaking, is not a research organization itself and at that time had a limited staff in Geneva. It was trying to address the public health problems that are shared by the nations of the world, and they were trying to get the best information they could by using expert committees to prepare reports. They had a very limited budget to actually finance original research.

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Reeves: The last time I heard, the World Reference Center for Arboviruses at Yale got something like \$10,000 or \$15,000 a year from WHO to cover their activity. That barely pays for some of their mailing costs. That reference center could not possibly be effective without extensive additional financing from the NIH, CDC, and the U.S. Armed Forces.

I want to make it clear that I personally want there to be a WHO because I think it's important, but I don't think that WHO in itself is the sole answer to all the health problems in the world. A lot of good research information has to be flowing through it constantly from the countries that can do it. WHO can implement or influence governments to utilize this knowledge to improve the health status of their populations.

Hughes: Does WHO have the prestige to put pressure on the individual governments to do whatever needs to be done?

Reeves: To a certain degree, but then they can't become too political. They are political in the sense that they're responsible to everybody who supports them. The ground rules they operate within are designed to handle that. So they have to be a force that moves things, but they cannot replace or dictate the format of health programs to any government. They can remind the United States or another government, "This is the international agreement that you approved." But they're even going to have to be somewhat careful about that if it is not done diplomatically. In other words, they can't say, "You're going to do this or stop doing something," as they might find their sources of funding cut off. There's a limit to how far they can go in following the advice of their scientific advisors, even though they may be the world's experts. WHO staff may have to say, "Well, that is a

scientifically valid recommendation for action, but that's not the way we can do it."

Hughes: I think we've been skirting around some important questions, but maybe you could bring it to some conclusion.

Reeves: I really feel it would be unfair for me to attempt to explain further the actions, problems, or successes of the WHO. I said earlier that my experience with that agency has been limited, and I have not been personally involved in their field operations or administrative activities. Anything further I would say would be based on reading of reports or heresay.

Reviewing Arbovirus Research in Latin America for the Pan American Health Organization, April-May 1962

Hughes: Why don't we shift over to your activities with the Pan American Health Organization?

Reeves: In 1962 a very interesting request came to me from Dr. Mauricio Martine da Silva of the Office of Research Coordination of PAHO, asking me to work with Dr. William Scherer, who at that time was professor of microbiology at the School of Medicine, University of Minnesota and whom I'd known for years. Dr. Hardy, who works with me now, was one of his doctoral students at Minnesota. Scherer had close relationships with PAHO. He was working on Venezuelan equine encephalitis in Latin America and was actually carrying one of their passports to help him get in and out of Latin American countries, to get supplies and equipment in, and to get specimens out to his home laboratory.

Anyway, the PAHO staff had made a decision that it would be worth their time to have Bill Scherer and me do a review of all the ongoing research and research needs on arthropod-borne virus diseases in Latin America, and I mean all of Latin America. They wanted us to go through Mexico, Central America, Panama, down the west coast of South America, up the east coast of South America, and to Trinidad to review all programs on arboviruses in any agency, whether it was a university, a federal government, or private foundations--in other words, review anything that was being done on arbovirus research and determine what the needs were to make that research more effective. Our review was to be completed by mid-June 1962, when it would be presented to the PAHO Advisory Committee on Medical Research.



Well, that sounded like an interesting assignment, even though it might be impossible to complete. I didn't have any idea what we were biting off or where all we were going to go. At the end of this one trip we had visited twenty-six laboratories that we or PAHO had been able to identify that were interested in arboviruses. We had a lot of very interesting experiences on that tour, and I won't try to review them all.

The main thing that we learned as we went around was that there were well-trained people, and they were widespread in Latin America. Most of these individuals had been trained in the United States. They'd come here for a couple of years, and they had worked in good laboratories under good people. A large proportion of these people had been trained as physicians before they had been selected for further training experiences in virology. Once back in their country, they were trying to do the type of research that was being done in the Rockefeller Foundation or university laboratories in the U.S.A. or abroad. And most of them had nothing but problems.

Number one, the salaries in their organizations frequently were not enough to live on. They had to have some other job-- private practice in medicine, running a pharmacy setup, doing something else besides research. They were moonlighting to make a living for the family. The result was that they really weren't working full time on arbovirus research, and anybody who has done such research knows it's a full-time job.

Number two, they were well trained and so on, but their staffs frequently were not adequately trained. They might have originally gotten a grant for equipment and supplies from some U.S. source or their government, but a lot of that equipment had broken down, and replacements couldn't be found in their country.

So really, almost every place we went, it wasn't a matter of their not knowing what the problems were or what they could or should do; it was a matter of their not having the resources. We were getting this information but didn't have any money to offer them. The Pan American Health Organization didn't have five cents to offer them. We wouldn't be at most laboratories very long before we found they were desperately trying to find out, "How much money can PAHO let us have?" We had to say, "We're not here for that purpose; we're here to obtain facts about what you need for the Pan American Health Organization." It wasn't easy. It wasn't easy at all.

Hughes: Who was on this group that was going around?

Reeves: Bill Scherer and Bill Reeves.



Hughes: Oh. [laughter]

Reeves: To continue, we also found that the local Pan American Health Organization staff would be very concerned about why we were there, what we were going to do, and what influence it would have on them individually. We had no responsibility to them or for them, and we were not attempting to evaluate their activities. Yet sometimes it was almost like they thought we were threatening their jobs; we must be doing something that was going to have some impact on them directly. We couldn't convince them that that wasn't the case. They'd want to take over our travel itinerary, change our schedules, or move our tickets to another airline. Finally, they'd want to take our traveler's checks and cash them for us at a great rate, which usually was less than we'd get at the hotel we were staying in. We had to sort of fight the system at the same time we were trying to work within it.

There was always a degree of a language barrier, because my Spanish is worthless and Bill Scherer at that time was still learning. He became very fluent later. We got into difficult situations because we understood too much Spanish for them to get away with discussing things behind our backs, but at the same time we couldn't carry on a conversation with them in the language they wanted to use. Then it got even worse for us when we went to Brazil, as they didn't want to talk Spanish; they wanted to talk Portuguese, and we couldn't. Fortunately for us, most of the scientists spoke English, and Bill and I were the handicapped persons.

It was a very interesting trip, and we met a lot of very competent people. We wrote a very detailed report.<sup>1</sup> I have no idea what influence it finally had on any individual research programs. Reputedly, the report evinced excellent comments and acceptance by the PAHO Advisory Committee on Medical Research.

Hughes: What did the report say?

Reeves: It told them what the needs and problems were, what the basic possibilities were of developing productive research if given adequate support. We didn't make any attempt to do a cost study or develop detailed plans for future research. But we felt there were a lot of problems that deserved intensive investigation and presented the facts on what obstacles there were.

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<sup>1</sup>W. C. Reeves and W. F. Scherer. Research and Research Needs in Arthropod-borne Virus Diseases in Latin America. 1962. Pan American Health Organization. Ref: RL-81/9, 28 May 1962.

The Pan American Health Organization had developed several laboratories in regional areas that they tried to finance. They had already done that to some degree, but primarily in enteric diseases and not in arboviruses.

What we found was that some of these laboratories weren't working too well. They would select a person who was a very competent scientist, employ him in the Pan American Health Organization, and send him to a country to help develop research programs and train the staff that was necessary. The person found out that he frequently had to train people from scratch and then design and develop a research program that would be productive as far as new knowledge was concerned. When work was done, they found the policy of PAHO was that they usually couldn't be the senior author on papers. As a matter of fact, in some cases they couldn't be an author on the paper; the local people had to be the authors of the papers.

Some of these people refused to stay very long in that sort of a position because their careers were going nowhere. That system didn't work, and I think our report to some extent told them that. You couldn't send out good scientists and have them not get the credit they deserved for what they were doing.

The other thing that we fell into a trap on was that we were trying to assess which arboviral diseases were problems. As an example, we were very happy to put in our report that, as far as we could tell, dengue was no longer a disease that occurred in Latin America and that *Aedes aegypti* eradication programs had been largely responsible for this. It appeared that the control programs for *aegypti* had been so successful that dengue fever was not a problem, and yellow fever as an epidemic disease didn't seem to be a real threat even though the jungle cycle was still producing cases in inland areas. Yellow fever wasn't spreading into urban areas.

We were pretty happy about that statement, and then to our chagrin within the next year data came in that there was an epidemic of dengue fever in Puerto Rico. At first that disease was reported as influenza, but it was dengue, and it was transmitted by a hellishly big population of *Aedes aegypti*. Scherer and I didn't go to Puerto Rico, because there was no laboratory or any research program there at that time. It turned out that the reports of there being no dengue or dengue-like disease and no or very few *aegypti* on the island were not right.

When that happened, representatives from the U.S. Army said, "We're going in there and abolish it. We will reestablish *Aedes*



*aegypti* and dengue control." I remember telling Colonel Buescher, who was the commanding officer of Walter Reed Army Institute of Research at that time, "Ed, you're going to have problems." He said, "What do you mean?" I said, "You don't know how to kill the mosquitoes." He said, "Oh, yes, we know how to do that. We will go down there with malathion and spray the island from top to bottom and get rid of them and of dengue." They went down there and sprayed the hell out of the island with malathion, and they found out, number one, that the mosquitoes were resistant to malathion and, number two, that the way they put out spray didn't get to mosquitoes in houses. So the army had to admit defeat. They didn't have the personnel to go house to house and control larval mosquitoes at their sources, as Gorgas had done in Cuba in the early 1900s or as Soper had done in South and Central America in the 1940s.

Hughes: Did you know that that was going to happen?

Reeves: No, but I guessed it. The fact remains that today they can't effectively control *Aedes aegypti* in Puerto Rico. The Center for Disease Control has tried with the best equipment, materials, and personnel they have. They just can't get insecticides into places where the mosquitoes are. It's a major problem that Dr. Duane J. Gubler and his group from CDC are working on with the Puerto Rico Health Department. In spite of their best efforts, dengue continues to be epidemic not only in Puerto Rico but over much of Latin America.

It was a very interesting experience for Scherer and me, because we soon realized that that world out there was operating on a completely different basis than ours is here, and that they had problems that neither we nor they could easily resolve. Many diseases pose major problems in those countries. Copacabana Beach may be a beautiful place, but you don't have to go very far into the surrounding area to find a lot of disease. In our survey we made no pretense that we knew the answers to the problems. We went out to try to find out what they thought their problems were, the research resources they needed, and we took that information back to PAHO.

We got back and were writing our report when a representative from WHO arrived in Washington who had gotten the word that we were writing this report. We found this guy looking over our shoulders as we wrote the report and telling us, "You can't write that. That's against WHO policy." This went on for a couple of hours one day. The Pan American Health Organization person whom we were working for, Mauricio da Silva, was getting very embarrassed. Finally I said to him, "What are we going to do about this?" He said, "Our visitor from WHO refuses to accept the



fact that the Pan American Health Organization is not a part of WHO. We were established before WHO by Fred Soper, and we're an independent agency. We collaborate with WHO in any way we can." I said, "He's saying we can't write this or can't say that because it doesn't fit WHO's position." Mauricio said, "Well, tell him it's none of his damn business."

I knew this guy from WHO very well, because I'd worked with him at WHO. I had to go to him and say, "We're not working for WHO now. Both of us have worked for you before, but we're working for PAHO now. We're not writing this report for WHO. When we are working for WHO, we'll write statements based on policy the way you want it. PAHO says that WHO's policy is not necessarily its policy, so get lost. You'll get a copy of the report." I got very angry. But no harm was done. He retired a few years afterwards.

Hughes: You wrote the report the way you wanted to?

Reeves: We wrote the report the way we had to, yes.

In 1967 I had an interesting assignment by PAHO to organize a meeting in Cali, Colombia, along with Dr. Bill Scherer, as a follow-up to the earlier tour we made around Latin America in 1962. The objective of this meeting was to stimulate scientists working on arboviruses in Latin America to organize a South American arbovirus committee, as we had done with the ACAV. It was also hoped to develop a newsletter in Spanish and a training center for technicians and field workers in South and Central America. We brought in four senior scientists from Latin America as a nucleus.<sup>1</sup> We held the meetings at a finca high in the Andes, which was a beautiful and isolated spot. We were nearing the end of the meeting when a messenger arrived with word for me that Charles Smith, the dean of our school, had died. I was selected to be acting dean, and Chancellor Roger Heyns wanted me to return to Berkeley immediately. I reluctantly left. Unfortunately, the agreements we had reached for a southern ACAV and training program never were implemented.

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<sup>1</sup>Pedro Galindo, Panama; Dr. Oscar Bruno Lobo, Brazil; Dr. Carlos San Martin, Colombia; and Dr. G. H. Bergold, Venezuela.

Advisor to the Pan American Health Organization on the Prevention of *Aedes aegypti*-borne Diseases, 1970-1972

Reeves: The interesting thing was that after Bill Scherer and I did that report in 1962, I was asked to make additional trips to Washington, D.C., to advise PAHO on a variety of problems. They continued to be very concerned about diseases transmitted by *Aedes aegypti* and how to control them. They seemed to think I had become the instant expert on *Aedes aegypti*, and I hadn't collected a hundred of them in my life at that time. I had read the literature and knew my limitations. In October 1970, I was asked to chair a PAHO study group that was to prepare a report.<sup>1</sup> The members of the group were very competent and represented many fields and regions.<sup>2</sup>

Equally and even more impressive was the list of scientific advisors<sup>3</sup> appointed to assist the panel. They represented a who's who of research on *Aedes aegypti*-borne diseases. In addition to those advisors, we had a team of seven scientists who formed a secretariat and who were equally competent advisors. Chairing a meeting with this group of individuals quickly taught me how it must feel to be a man thrown in a cage to manage a mob of lions and tigers, with a chair as his only protection. However, once we worked our way through the hidden agendas of the individuals and the organizations they represented, we found a solid consensus of objectives and recommendations that could be presented to PAHO to minimize the occurrence of these diseases.

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<sup>1</sup>Report of the PAHO Study Group on The Prevention of *Aedes aegypti*-Borne Diseases. 1970.

<sup>2</sup>Dr. George Foster, Prof. Anthropology, Univ. of California; Dr. William McD. Hammon, Prof. Epidemiology, Univ. of Pittsburgh; Dr. William W. MacDonald, School of Tropical Medicine, Liverpool, England; Dr. David Orellana, Chief Officer of International Health, Ministry of Health and Welfare, Venezuela; Dr. Antonio M. Vilchis, Director, National Institute of Microbiology, Argentina; and Dr. Abel Wolman, Emeritus Prof. of Sanitary Engineering and Water Resources, The Johns Hopkins Univ.

<sup>3</sup>Dr. Solón de Camargo, National Dept. of Rural Endemics, Brazil; Dr. Wilbur G. Downs, Director, Yale Arbovirus Unit, Yale Univ.; Dr. Scott B. Halstead, School of Medicine, Hawaii; Dr. Philip K. Russell, Dept. of Virus Diseases, Walter Reed Army Inst. of Research; Dr. James V. Smith, Special Assistant to the Director, National Communicable Diseases Center; Dr. Fred L. Soper, Emeritus Director, PAHO; and Dr. Adrian T. Muñoz, Chief of the National Antimosquito Campaign, Mexico.

I remember at one meeting a gentleman from Mexico wanted to make it against the law for the United States to export used tires to Mexico because they were carrying *Aedes aegypti* eggs down to Mexico and reintroducing them. He said that the United States ought to have to sterilize all these tires before they were moved in. I said, "There's another way to approach this problem. Why don't you make it against the law for the tires to be brought into Mexico?" He said, "We have to have the tires to make shoes and things like that, so we can't stop it. I said, "I don't think we can solve that at PAHO. That's a problem to be resolved between governments. PAHO doesn't write laws, rules, or agreements between governments."

At that same meeting where I was presiding, a gentleman from Brazil said that they didn't have to worry about mosquitoes being reintroduced into Brazil because it was against the law, so they wouldn't be introduced. I said, "Look, if entire automobiles are being smuggled into Belem, Brazil, how do you expect to keep out mosquitoes?" I had just come back from Belem, Brazil, and the speaker had never been there.

In 1972 I was asked to prepare and present a paper on "Recrudescence of Arthropod-Borne Virus Diseases in the Americas," in a special symposium before the PAHO Advisory Committee on Medical Research. A series of six papers was given and published<sup>1</sup> covering the major vector-borne diseases and their control.

Hughes: How important is PAHO?

Reeves: It is like WHO. The Pan American Health Organization has a very appropriate task to perform, and they wouldn't be there if the various governments hadn't agreed to its importance and the necessity for its activities. But PAHO at times has very marked limitations on what it really can do, and it's not primarily a research organization. It's a political organization that is trying to bring together the interests and solve the needs of a wide variety of countries. I think what it does is important. As an example, our committee in 1970 recommended that a study be made of the costs of *Aedes aegypti*-borne diseases for the area within PAHO's jurisdiction. This study was completed that year.<sup>2</sup>

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<sup>1</sup>PAHO 1972. Vector Control and the Recrudescence of Vector-Borne Diseases, Scientific Publication NG 238, pp. 1-85.

<sup>2</sup>Cost-Benefit Aspects of Preventing *Aedes aegypti*-Borne Diseases in the Western Hemisphere, Robert R. Nathan Associates, Inc., Wash., D.C., Sept. 28, 1970.



Venezuelan Equine Encephalitis Epizootic in Texas, 1971

Hughes: What was your role in the Venezuelan equine encephalitis epizootic of the seventies?

Reeves: Venezuelan equine encephalitis had worked its way up through the Central American countries in 1969 and into Mexico by 1970. They were not vaccinating horses. The U.S. Army had developed a good vaccine (TC83) to prevent cases in humans or equines, but it wasn't being used at the necessary levels. The disease had appeared way down in the southern part of Central America in 1969 and probably had come in from South America. It kept moving northward into Mexico; then it spilled over the border into Texas in 1971. The limited efforts to stop it had been unsuccessful and did not even recognize that it was going to be a problem. Some of us were yelling to do something about it much earlier.

Now, the Pan American Health Organization isn't the principal agency that deals with a disease like this, because it is primarily a disease in equines and of lesser importance as a human disease. However, PAHO had a concern and dealt with it in part, because it was infecting people in Central America and Mexico. When the disease appeared in Texas, the U.S. Government declared it an emergency and spent millions of dollars to control it. I became involved because there was concern that it might come to California. I attended meetings, and decisions were made to vaccinate all the equines in California. In addition, I had a doctoral student, Dr. James Ferguson, who evaluated the effectiveness of the VEE vaccine in California horses for his Ph.D. thesis.<sup>1</sup>

In 1971-1972 I had resigned as dean of our school and was on sabbatical leave in residence to get back into field research. I never did the work I had planned because of the many meetings, state and national, on the Venezuelan encephalitis problem. As examples of activities at that time, in July 1971 I was appointed to a special committee to be advisory to the California State Bureau of Animal Health and the State Department of Health Services. This committee was concerned with development of a plan to vaccinate all equines in the state, including zebras in the zoos. Shortly after that, Dr. J. L. Kendrick, Vice President for

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<sup>1</sup>J. A. Ferguson. Epidemiological and Immunological Studies on the Alternated Venezuelan Equine Encephalomyelitis Vaccine Virus (TC83). 1974. Ph.D. thesis, University of California, Berkeley.

Agricultural Sciences in the University, and Dr. L. Saylor, the state health officer, called me to a meeting to advise them on research needs relevant to the Venezuelan epidemic action.

Then the Veterinary Virus Research Branch of the U.S. Department of Agriculture added me to their advisory group on the VEE epidemic. It seemed to me that there was no end of meetings and organizations concerned with the problem, as the Pan American Health Organization and CDC also were asking me to meetings on VEE.

Karl Johnson worked on the virological aspects of the problem at the Middle America Research Unit in Panama. Various CDC people were working on it. The people concerned from the various countries were all brought together in Washington, D.C., by PAHO to review the problem. Pedro Galindo came up from Panama for one of those meetings. He actually had a heart attack while making his presentation. The situation had its morbid humor, because when he woke up he realized that nonpracticing physicians like Bill Scherer, Phil Russell, and Karl Johnson were all huddled over him. Pedro knew they hadn't really practiced medicine since they graduated from medical school and internships. Pedro said, "Please take me to the hospital--not you guys." [laughter] And they did.

The Pan American Health Organization worked with the various veterinary agencies to coordinate and exchange information on control efforts and reporting of the disease.

Hughes: You mentioned to me off tape that you were an advisor to PAHO for over thirty years?

Reeves: Yes, it began in the mid 1960s, and I still am. I just had notice that PAHO and WHO were renewing my appointments, but they have not asked me to do anything for many years. I think they keep people like me on as advisors because they've got a name of a person recognized as working in the field, and they figure that if you're listed, you are still going to be available if they really need you. I suppose it's possible they also do it because it's easier to keep you on a list than it is to drop you off. I don't know.

#### Gorgas Memorial Laboratory, Panama, 1958-1991

Hughes: All right. The next topic is the Gorgas Memorial Laboratory.

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Reeves: The Gorgas Memorial Laboratory was established in the very early 1930s by the U.S. Government in an agreement with Panama. It was done in recognition of General Gorgas, who had controlled malaria, yellow fever, and other major diseases, and this allowed them to complete construction of the Panama Canal. Panama is a crossroads of commerce in the Americas. The two governments agreed there was a need for research on diseases unique to that region of the tropics. The laboratory under the direction of Dr. Herbert Clark rapidly established a reputation for outstanding research. Pedro Galindo, who had worked with me in Kern County in 1943 and had been a fellow student at Berkeley, was the entomologist for the laboratory, and Dr. Carl Johnson was a physician and tropical disease expert. Carl had become the second director, succeeding Dr. Clark. He was called C-Carl Johnson to differentiate him from K-Karl Johnson, Director of the MARU laboratory in the Canal Zone.

In September 1958, I was coming home via Trinidad from the Tropical Medicine and Hygiene meetings in Lisbon. When I finished reviewing the activities of the Rockefeller Foundation program, I wrote a report for Wil Downs. The Rockefeller Foundation did not object to my taking a plane through Panama to return to California. This gave me a chance to visit my friend Pedro Galindo and see a location famous for historical studies on tropical diseases.

The director of the Gorgas Lab during my visit was Dr. Carl Johnson. I knew Carl Johnson because I'd met him at tropical medicine meetings. I walked in, and he said, "What are you here for?" I said, "I'm on my way home and just thought I'd come in here and see you guys and spend a couple of days. Is there anything I can do for you?" He said, "Yes, there sure is." I said, "What's your problem?"

Carl said, "Well, for one thing, we don't really know what's going on in one of our laboratories. Pedro's been collecting all these samples of mosquitoes for virus isolations, and currently he has this horrible headache that clinically is encephalitis. We send all the diagnostic specimens from Pedro and his mosquitoes to our virus laboratory, but the lady who's in charge won't tell me what she's found in the samples. I sure would like to know; maybe you can find out."

I thought this was sort of an interesting situation, and I said, "Who is it?" He said, "It's Dr. Enid De Rodaniche." I knew her name from the literature, but I'd never met her. Carl said, "If you could possibly get down to her laboratory, find out what she's doing, and tell us, it would certainly be appreciated." This is the director of the Gorgas laboratory and his head



entomologist, Pedro, asking for help. I said, "I can take a crack at it." He said, "We'll take you down and introduce you." I said, "No, let me handle this from the word 'go.' I don't want you to introduce me; because if you introduce me, she probably won't give any information to me." So he said, "Okay," and they pointed out where her place was. Her lab had a glass partition that went up partway, so you could see into it from the hallway.

I went down, and I'd never seen this lady before. She was the only person in there and appeared to be a nice Panamanian lady. I walked in, and she looked at me. I said, "You know of me, Dr. Rodaniche; I've requested reprints from you, I've read your reprints on yellow fever, and so on. I'm Bill Reeves from California. I had to come by here and visit you." She said, "Oh, fantástico." I said, "Let me tell you what we've been doing." I started telling her about all our research. I didn't give her a chance to get a word in edgewise, and she was getting more and more nervous all the time. [laughter] She was learning all about California, but she probably couldn't care less about California. Finally I said, "Do you suppose I can get a cup of coffee?" She said, "Yes, yes, I'll get you a cup of coffee." When she came back, she said, "Now, let me tell you what I'm doing." [laughter] She didn't even let me thank her for the coffee.

The next thing you know, she has all of her lab workbooks out, and we're drinking coffee and having a big old time. I finally said, "Do you mind if I take some notes?" I had learned this from Dr. F. C. Bishop. Remember him?

Hugues: Yes, he'd taken notes on your first thesis, around 1940.

Reeves: She said, "Sure, go ahead." To make a long story short, she showed me the diagnostic workup on Pedro, and it proved he had St. Louis encephalitis, which was very nice to know. He was the first case from Panama. He was fine; he was alive and well by now. Then she went through all the virus isolation she'd made from the mosquitoes and the serological work on blood samples collected from the jungles. I spent a half a day in her lab, and I was copying down all this stuff. Finally I said, "I have to apologize to you; I can't spend any more time here. I have to do something for Carl Johnson tomorrow morning, and I don't know what it is." She said, "Would you take some specimens back to Berkeley with you and verify my work?" I said, "Oh, yes, I'd be glad to. Get them lyophilized and packed up, and I'll take them back when I go. I have permits with me."

Meanwhile, I'd been seeing Carl Johnson's and Pedro Galindo's little heads popping up, looking over this partition to see how I'm doing. [laughter] I'd made sure her back was to them. So I

go down to Carl's office, and I say, "Well, gang, here you are. Here's everything she's been doing. Pedro, you've had St. Louis encephalitis. These are the viruses out of your mosquitoes, and I'm taking specimens to Berkeley to verify her findings." They said, "That's fantastic. Now we can write our annual report."

Hughes: Why was she being so secretive?

Reeves: That I can't explain, and I didn't ask her, as it would not have helped their relationship with her.

Then I found out from Carl and Pedro what my assignment was for the next day. Alexis Shelokof had been sent to the Canal Zone by the National Institutes of Health of the USPHS to establish a new Middle America Research Unit. This was done without any real discussions with Carl Johnson, but in his view a new laboratory to study tropical diseases was going to be established within a few miles of the Gorgas. Alexis was an excellent virologist and has become a very good friend of mine. As a matter of fact, he's now on this committee of the National Academy of Sciences that I'm on with regards to the emerging biological agents that may cause health problems in the U.S.A.

Anyway, Alexis had been sent down there as a Public Health Service commissioned officer to establish this laboratory, and people were going to be coming down from NIH to join him. As far as the Panamanians and the Gorgas staff were concerned, this was an invasion of their research territory and their domain. The new lab would be in the Canal Zone, but it was almost across the street from the Gorgas.

The real problem was that the Gorgas staff weren't sure they wanted to collaborate with Alexis, but they also recognized that he hadn't initiated the development but was assigned to it. I hadn't met Alexis before this time. The immediate problem was that Alexis Shelokof had told Carl Johnson that Dr. Joe Smadel would be in town the next day, and he wanted to talk to Carl about the new lab. Joe Smadel was a powerhouse, later the research director of the Institute of Allergy and Infectious Diseases of the National Institutes of Health. This guy is a real steamroller. I mean, he's about as tough as they come. Another researcher from NIH whom I hadn't met was also coming, but I knew him by name, and that was Ralph Muckenfuss. Ralph was another big powerhouse at the National Institutes of Health.

These people were coming to make their contacts with the Minister of Health of Panama and to get everything ironed out politically. Carl says to me, "Those guys are coming over here tomorrow morning, and I'm scared. I'd like to have you be in the



room when they come in. I'm really scared of these guys. I think they may try to run us out of business." I said, "Sure, I'll be here. That will be interesting." Because I knew Joe Smadel. I didn't know Shelokof at that time.

Those guys walked in the next morning, and Pedro Galindo, Carl Johnson, and I were sitting in the room. All of a sudden they saw me, and Joe Smadel said, "What the hell are you doing here?" I said, "I'm on my way back home from Lisbon and Trinidad. I just left Trinidad, and I just happen to be here. Carl said he thought you would like to see me and asked me if I'd sit in on this meeting."

Well, it sort of changed the complexion of the meeting a little bit, because this wasn't necessarily making their job any easier. I could see Joe thinking of how he could get rid of me, but he couldn't; I was a guest of the Gorgas Laboratory. There wasn't a lot of blood shed that morning, and they rapidly came to a preliminary agreement that the NIH would do most of their work outside of Panama and that there would be close collaboration on some studies. I could see Alexis Shelokof feeling better and better all the time. It turned out that he had ulcers, and his new job wasn't helping his ulcers at all. I think it actually helped to have an outsider there, and an outsider they knew wouldn't hesitate when he got home to talk about it.

My first experience with Joe Smadel had been when I did my earlier review of all medical entomology programs in the U.S. Army, including Walter Reed Army Institute of Research, for which he was the director of research at that time. We had some differences of opinion at that earlier date but had worked them out. So the Gorgas and NIH units, headed by Carl Johnson, and Karl Johnson at a later date, maintained relationships with each other in following years, and 1959 was its beginning.

When Bill Scherer and I started going around Latin America for PAHO, the Gorgas Laboratory was one of our frequent stops. That was in 1962. By that time, Martin Young, who was a malariologist, had taken over the directorship. Karl Johnson and his sizeable staff were over in MARU.

We went through the same survey questions with the Gorgas group that we did with the others in Latin America. Now, obviously, the Gorgas was in much better shape financially because they had an annual budget from the U.S. Congress. The two types of laboratories in Latin America that were in good shape were the Gorgas Laboratory and the laboratories that still had a base of operation from the Rockefeller Foundation. The Foundation had a laboratory in Cali, Colombia; Dr. Robert Kokernot was their man



there, and Dr. Carlos San Martin, a well-trained person from Colombia who had been trained in the Rockefeller Foundation, was the laboratory director. So they had good laboratories with a good, solid base and field projects.

At the same time, the Colombia government program in Bogotá was not going well at all. They didn't have the funding, and they didn't have much going for them. They had a yellow fever vaccine development program that was going. The Rockefeller Foundation had a second laboratory in Bogotá, which Dr. Ron McKenzie was in charge of. He had been one of my students here. He was doing studies in the interior of Colombia, trying to isolate viruses. His program was fairly independent from the one in Cali, Colombia.

Member, Scientific Advisory Board, and Director Emeritus, Gorgas Memorial Institute, Washington, D.C., 1965-1991

Reeves: So that was my second visit to Gorgas.

In 1965 I was asked to become a member of the Scientific Advisory Board for the Gorgas Memorial Institute, which is the parent organization in Washington, D.C., for the Gorgas Memorial Laboratory in Panama. The Panama laboratory was the only laboratory they had, but to make policy they had a governing board of trustees, including the Surgeon General for the USPHS, the head of the Ministry of Health for Panama, and many big names in tropical medicine, and then they had a scientific advisory board.

In January 1973, Pedro Galindo was the director of the Gorgas Memorial Laboratory, and he asked me to come down and review their arbovirus program. I spent a week in the field with Pedro, visiting field projects and making recommendations for various approaches. In 1976 I was asked to serve on a committee the Fogarty Center was sending down to review the program in Panama. At this time the Gorgas budget was being defended in Congressional hearings by the Fogarty Center, as the budget was a part of the National Institutes of Health budget. They said, "There's no need for people to come up here from Panama to defend their budget in Congressional hearings. We're here; we'll do it for them." So as a part of supervising and handling their budget, they insisted that a scientific group go down periodically and review the program. Again, it was a very interesting committee. Dr. Vernon Knight, who was an excellent virologist from Baylor University, was the chairman; Dr. William Foege, who was very much involved at that time in the worldwide smallpox eradication program and was later to be the director for the Centers for Disease Control, was

an advisor to the committee. John Scanlon, who had retired as head medical entomologist from the U.S. Army, and several other people were on the committee, including one of my former students, Phil Winters, who was in the army and was on the board of directors for the Gorgas Memorial Institute.

We spent four days reviewing the Gorgas program and making recommendations for changes that ought to be made in personnel and administration and so on, and we went out in the field and saw a lot of their programs there. I made three or four additional trips in the 1970s and later. When my son joined the staff of the Gorgas Laboratory, I said I wouldn't go down any more to do reviews because I didn't think it was right. Some of the people didn't understand that; but my son did, and I did, and I didn't go back again. I made additional trips to the Middle America Research Unit with Karl and to go out fishing for marlin, sailfish, dorado, and things like that, but I didn't make any more visits officially to the Gorgas Lab.

Another time when I got involved with the Gorgas Memorial Institute in Washington, D.C., was in 1980, when they came to me and asked if I would prepare a paper to memorialize General William Crawford Gorgas on the occasion of the fiftieth anniversary of the Gorgas Laboratory in Panama. They asked me to write a paper reviewing Gorgas's career and how I saw the Gorgas Laboratory would fit into General Gorgas's legacy of disease control. It was actually published as a supplement to the Tropical Medicine and Hygiene News,<sup>1</sup> so it got wide distribution. It was fun to do.

My manuscript was sent over to a person who was teaching the history of medicine at the Uniformed Services University of the Health Sciences in Bethesda, to make sure my facts were right. I didn't particularly appreciate the lack of confidence, but he didn't find anything wrong. I told them I didn't think their action was exactly a compliment. Why in the hell didn't they ask him to write the paper? Anyway, they said in apology that he didn't understand Gorgas as well as I did. I thought the paper gave me a chance to give a lot of support to the laboratory as far as its future was concerned.

My son went back down to Panama in 1977 to join their research staff as chief of the Department of Virology/Epidemiology. He was made the director of the laboratory in

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<sup>1</sup>William C. Reeves. William Crawford Gorgas: A view of his contribution to control of selected disease problems in the Americas. Trop. Med. Hyg. News, 1980, 29 (supplement).



August 1987. At that time my appointment was changed to director emeritus by the Gorgas Institute. The title has never been explained to me. My son was there until they closed the laboratory just before the U.S. armed forces occupation in 1991. At that time he moved up to CDC in Atlanta.

#### Termination of Gorgas Memorial Institute and Laboratory, 1991

Reeves: Meanwhile, the National Institutes of Health had not put in a budget for the Gorgas Institute, which meant the Gorgas Laboratory wasn't funded anymore, and that was one of the reasons Gorgas Laboratory was closing. The NIH took that money as part of their program to support international health research. They put out an announcement that there was to be competition for that amount of money, and that any university or any research agency that wanted to could apply for any part of the money. That really killed the Gorgas Institute as a supervising and policy group for the lab, and it closed the lab. I think this was very unfortunate, because there's a real need for a laboratory there. Now the Panamanians and the Panamanian government are trying to reinstitute a Gorgas Memorial Laboratory under a completely different board of trustees and financial setup. I hope it succeeds.

So we've gone in a big circle. Started sixty years ago, the Gorgas Memorial Institute and Laboratory are dead in 1991. It was a big circle and the end of something that was very important. Some people said it no longer was useful, and I don't agree. The laboratory was getting out of the rut of working on nothing but a limited array of parasitic diseases--malaria, yellow fever, leishmaniasis, and trypanosomiasis--and broadening out into new infectious disease problems, indeed also getting into chronic diseases. A program that my son started on cancer and other basic health problems in Latin America was opening new and unique areas of study on chronic diseases in an underdeveloped country.

Hughes: Is there anything else in Panama?

Reeves: No, that's the only research group in that country concerned with tropical diseases or diseases in native people. There is a ministry of health, and there are things like the cancer hospital, but there are no other major research activities except basic biological studies such as the Smithsonian Institution. A number of people from the United States were using the Gorgas Laboratory as a base for their studies: Dr. Jacob Fraenkel from Kansas City works on toxoplasmosis, and one of his major field projects was out of the Gorgas Laboratory. I don't know what has happened to



that; I haven't seen him since it's happened. The army is very concerned. The army continued to support an activity there, because the largest colony of primates that are susceptible to human malaria and that are essential for malaria vaccine evaluations is there at that lab. It is not a monkey colony that can easily be moved to some other place, so they still kept staff there. Money came in to maintain that colony because it's an important resource for the international program on malaria vaccine development. I think that has stopped now.

I guess as a last point I should admit that I personally will miss possible visits to Panama. It was a different experience. Whenever I was on a field trip I was overcome by a feeling of the vastness of tropical rain forests. It made me feel small. As a matter of fact, I'll never be comfortable in heavy jungle like in Panama. I feel lost there. I mean, I grew up in the western United States where I always could see a mountain, a reference point, and I could see what's in front of my nose. If they just dropped me in the jungle with a canopy a hundred and some feet above me, and I couldn't see where I was and couldn't see the sun, I'd have no sense of direction; I'd be lost. The people who have grown up there, like Pedro Galindo, are at home. My son and his children, who grew up down there, feel completely at home in the jungle. But I'm scared. I won't let somebody out of my sight if they know where they are. And it's a very difficult place for me to work in. The mosquitoes are harder to find, there are fewer of them, and there are a lot more species there than in California. But the people who have grown up there and have lived there do very well at learning how to work in such places.

Review of the Division of Infectious Disease Epidemiology,  
Department of Public Health, Yale University, 1989##

[Interview 10: May 1, 1991]

- Hughes: Would you say something about your review of the Yale Arbovirus Unit, please?
- Reeves: One of the interesting things that you get asked to do in my position is that you suddenly get a telephone call saying that a review is wanted either internally by an organization or is being requested by some other organization for accreditation of a program or review of their research. In this particular instance, Dr. Tom Monath and I were asked by Yale in November 1989 to go to to their Department of Public Health and Preventive Medicine to review a subunit, the Division of Infectious Disease Epidemiology.

Included in that group is the Yale Arbovirus Research Unit, but they don't usually call it that; it's simply called YARU. It's the unit that was formed by the Rockefeller Foundation.

When the Rockefeller Foundation arbovirus research group had to get out of their laboratories in New York, they moved to Yale in 1964. The foundation erected a building to contain the Department of Preventive Medicine and Public Health, the equivalent of a school of public health. The unit is part of the medical school, so it had to be called a department rather than a school. They have a chairman of that department, but he or she functions as a dean would in a school of public health.

Anyway, YARU was formed by the Rockefeller Foundation and located in the building which they constructed, and it occupied several floors of the building. Dr. Wilbur Downs was very much involved in this, because he was in the key position in New York at the Rockefeller Foundation at that time. This research unit was dealing with highly infectious diseases, so it had to have maximum security. There were all sorts of closed doors, signs up about dangerous agents, and so on. Some people in the department don't really understand what goes on behind those doors and are sort of worried about it. In short, they were not sure they wanted to be in the same building. The people who were frequently behind those locked doors were doing their research, but they also came out to participate in teaching, as they were in the Division of Infectious Disease Epidemiology. Anyway, interrelationships of the various groups in the department were not going exactly as was desirable in an academic unit, so they asked for a review to be done to see whether the work that was going on there was important. With budget crunches and so on, they wanted to know to what extent the administration should really be strongly supporting this organization.

Well, I have a very strong loyalty and relationship to the YARU that started with the Rockefeller Foundation relationship as early as the 1950s, so I felt very dutybound to participate in this review. Many old associates were in the YARU. People such as Wilbur Downs and Thomas H. G. Aitken were retired from the foundation but were still active in research and teaching, and I had known them for over forty years. Dr. Robert Tesh was there, and I had known him from earlier visits to MARU in Panama, when he was doing pioneer work in sand flies and viruses. Dr. Robert Shope was director of YARU, and I had been associated with him for many years. Actually, it turned out they only asked two of us to do the review: Dr. Tom Monath from the Fort Detrick Army Research Unit in Frederick, Maryland, and myself. Tom is very much into arboviruses, as I am.



We reviewed the program; they were doing an excellent job and had all the usual difficulties of not having enough money. Yale faculty members are expected to have research grants to bring in most of their salaries, even though they're still expected to teach and do all the other things expected of a professor at the same time. It's a particular hardship on the younger faculty members. They come in as assistant professor, and either they sink or they swim--they get research grants or do not--so there's a lot of competition between them.

It was a very interesting review in the sense that the staffing was doing very good science up and down the line on a whole variety of diseases that were of interest to me and to Dr. Monath. It also became very obvious that it was only going to take a little bit of conversation to really make things work out much better between the YARU group and the rest of the people in that school. Unfortunately, the head of the department, Dr. Burton H. Singer, wasn't there at the time we made the visit. He was someplace in China.

So we wrote a report outlining what we saw as the opportunities for this group to make real contributions to the department in teaching and committee work as well as research, and also how they should and could be related more closely to activities in the medical school.

Hughes: Was the problem the feeling that the unit wasn't contributing to the department?

Reeves: Well, they weren't as close a part of the overall department as some people would like to see. The other faculty didn't quite know who they were or why they were there, plus the fact that they were utilizing a lot of space. The YARU group was extremely defensive, because a previous department chairman had taken quite a bit of space away from them, which the original Rockefeller Foundation contract said would never happen. In time these agreements sort of lost their punch, so that people from the medical school were getting some of the space that the YARU group thought was theirs.

It was just one of those situations where having outside people come in can really help unscramble problems that the people internally think they're facing but perhaps are not communicating about. I think it was a worthwhile visit to make. It's a lot of work, because you have to talk to everybody, listen to everybody's problems that they either have or think they have, and then try to come to some sort of an agreement. We spent four or five days there and had to write a report.



Hughes: Did it work?

Reeves: As far as I can see, at this stage it did. I haven't been back. I've talked to some of the people involved. But you always wonder what you have accomplished, whether it's a consultant assignment with the army or review of an academic program.

Earlier I'd been to Yale on several occasions at the invitation of the National Institutes of Health to review some of their research grants and programs on arthropod-borne diseases. Again, somebody had to go there who would understand what they were doing, because they were running the World Arbovirus Reference Center for the WHO, even though the WHO really wasn't contributing significant money to them. The money they had almost all came from the U.S. Government, either the Public Health Service or the armed forces. There was a need for an understanding review by outside individuals to make sure they were supported the way they had to be, or they couldn't function. I'd made several visits to Yale in that context before. I knew most of the people involved in their Department of Public Health, because when I was dean of our school I knew their former department chairman, Dr. Robert W. McCollum, and other staff very well and had worked with them on committees. Also, several of our former students were on their faculty, so I knew the people well. It was an easy and sort of a fun assignment, but it was a lot of work.

#### Review of the School of Public Health, University of Hawaii, 1973

Reeves: I was asked some years earlier by the Association of Schools of Public Health and the American Public Health Association to visit the school of public health in Hawaii to review them for accreditation. On this occasion in 1973, the president of the University of Hawaii, Dr. Cleveland, decided he had a problem in the school of public health. He wasn't quite sure what the problem was, but it was obvious that the administration of the school, the faculty, and the students were not agreeing on everything. This was creating some administrative problems for him, so he wanted outside people to come in. They invited me, as the former dean from this school--that was back in 1973--and the former dean from North Carolina, Dr. Fred Mayes, to visit, talk to the various people involved, and see if we could help the president of the university unscramble the problem. When we talked to the president, he said, "If they create any more problems for me, I'm going to close the school. So I hope you

gentlemen can solve the problem. I don't want to close the school."

We found out indeed that some of the young faculty were getting pretty excited about their ideas on how the school should be run, which weren't really compatible with what the dean, Dr. Ed O'Rourke, or what the president of the university thought. The students were siding with the young faculty, as they frequently do. The resolution turned out to be very simple--mainly that we sat down with the young faculty and the students and said, "Look, you can go on making all this noise and causing this difficulty, but if you continue you're not going to be here anymore, because there won't be any school of public health. Now, which do you want?"

When this came from outside people, they had to listen to us. They had thought before that it was a bluff. I said, "I've talked to the president of the university, and he said there's no question in his mind what he's going to do. He's not going to put up with it anymore. Either quiet down and try to solve the problems internally without making so much noise, or else."

Again I was dealing with people, some of whom I'd had here as former students; so I knew some of the young faculty and found it was not difficult to talk to them. Some of these people were just having fun making trouble.

Hughes: Without realizing the consequences.

Reeves: Without realizing or perhaps caring what was going to happen.

Hughes: Do you think you would have been asked if you hadn't been a former dean?

Reeves: I think at this particular point in this particular situation they wanted two former deans from well-established schools, and they wanted the people being reviewed to know who the visitors were, what they were doing, and that they had a knowledge of what a school of public health ought to be about. There were problems on both sides. There always are in these situations. As a matter of fact, the dean of the school of public health, Dr. Ed O'Rourke, resigned resigned shortly thereafter and was replaced, but not entirely because of these problems.



Member and Chairman, Vector Control Advisory Committee, California State Department of Health Services, 1948 to Present

Hughes: All right. What about your work with the Vector Control Advisory Committee of the State Health Department?

Reeves: We talked earlier about some of the problems the Vector Control Section--as it was called originally--of the State Health Department had had with mosquito control districts. I think we've also talked about the problems we had when we had epidemics of encephalitis that threatened the state.

The State Health Department really got reorganized and staffed after World War II. We had a threatened epidemic of encephalitis in the late 1940s, and they found it would be an advantage to form what they called a Vector Control Advisory Committee that would be advisory to the director of the department. This committee included representatives from the university. People like Dr. Meyer were on it, I was on it, Hammon before he left was on it, and people from the mosquito control districts and local health departments were on it. The purpose of the committee was to do annual reviews of the vector control program in the State Department of Health--which it was called then; that was before it became the State Department of Health Services--and to advise them whether they had adequate programs on encephalitis, plague, relapsing fever, Rocky Mountain spotted fever, malaria, and whatever other vector-borne diseases there were in the state of California. Encephalitis, malaria, and plague really commanded the major attention.

The advisory group would review what they had in the way of staff and programs. At that time they had a fairly extensive research program on mosquito control that was taken over later by the university. We had to go through their programs step by step and advise them on whether we thought they were doing the right things. In the event of an epidemic, we had to become spokesmen, to advise them on what steps they might take to abate the epidemic, or to endorse the steps they were already taking.

I got into these activities when they started back in the late forties, and, to my amazement, the next thing I knew I was running the meetings. I had an appointment from the director of the Department of Health to be chairman of the committee and consultant in epidemiology, and it was an interesting experience. I had to learn quickly to run a meeting. Also, I frequently had to meet with the press after the meetings and tell them what the problems were and defend the positions that we were taking. We had a meeting in Sacramento about a month ago of the Vector Control Advisory Committee. I'm still the chairman after forty



years, and this has been without a break all these years. I frequently have offered to get off the committee and be replaced, but they just say they've gotten so used to my being there that they'd rather leave it the way it is.

When there are groups or individuals who for one reason or another oppose actions that are recommended by the department and the committee, it becomes a pretty heavy responsibility. We've had meetings when epidemics were in progress in 1952, 1958, '59, and the sixties, and we were recommending that airplane spraying with insecticides should be done to control virus-infected adult mosquitoes over large areas, including urban areas. People would get up from the audience and say that they were against this, and that they thought it was a risk to the environment and illegal to do so. They were opposing the program very strenuously. When this happened, it just came down to the point of having to tell these people in a public meeting, "This is the only thing we know that will abate an epidemic. We have to decrease the risk of further exposure to disease. If you want to take the responsibility of saying it can't be done and making a stand, you take the responsibility for an epidemic and for further cases. Don't ask us to." Usually the opposition pretty well evaporates at that time.

These advisory groups barely are kept in place during times like now, when there are budget crunches, the state organizations are losing staff, and there hasn't been an epidemic for several years. Two years ago we went to the state director of health services, Dr. Kenneth W. Kizer, and told him that in the opinion of the advisory committee there really needed to be an increased surveillance program for Lyme disease in California. This disease was emerging as an increasingly important problem, and we were getting to know enough about how it was spread and so on that we thought that an intensive surveillance program such as we had for the mosquito-borne viruses was highly justified. We were successful in that. He managed to get five new positions assigned: two in the bacteriology laboratory, two in the vector control section, and one in the epidemiology section of the State Department of Health Services. So five new positions were created for this purpose. We felt this was quite a victory, because budgets were not really that loose.

Unfortunately, two years later, in 1991, those five positions were eliminated from the budget by the state legislature and the governor because of the financial crisis. So for two years we had a surveillance program initiated, and it was doing quite well. It greatly expanded the area in which we know Lyme disease occurs in California and expanded the knowledge of where the tick vector occurs. The program was working closely with the Centers for

Disease Control on the new diagnostic criteria for Lyme disease, the need for additional new and better laboratory diagnostic techniques, and so on. It's rather discouraging when two years after you get something like that started it's abolished because of budget cuts, but that's the way life is today.

We've had periodic outbreaks of malaria in California. This disease was declared eradicated from the state in the 1940s, and now you suddenly have a small epidemic that draws attention back to the fact that this disease potentially could become reestablished in California. It is transmitted when it's reintroduced by immigrants or travelers from overseas or by laborers from Mexico coming here who are infected. The local *Anopheles* mosquitoes bite them and then go on to bite a local person. Temporarily, the disease is no longer eradicated from California. So we don't know quite what to expect in the future. If budget crunches go on, we might again have a lot more malaria in the state. It used to be a very highly endemic disease, all through the Sierra foothill areas. They couldn't develop the area around Bakersfield until they had developed a mosquito control program for malaria in the early 1900s.

Hughes: Are these committees multidisciplinary?

Reeves: Yes, this is particularly true now in the sense of the organizations that they represent. The Vector Control Advisory Committee used to be composed primarily of people who represented medical and entomological knowledge of the diseases and health agencies that were concerned with control of diseases like encephalitis, malaria, or plague.

In recent years, state laws have changed so that the committees are limited in how many representatives of the various sciences or organizations they can have. There is an increase in the degree to which they have to represent state agencies that have overlapping concerns, not necessarily with the diseases but with environmental or regulatory concerns. For example, State Fish and Game now has to be represented on the Vector Control Advisory Committee, and the reason is that there are threatened animal species that State Fish and Game is responsible for and that are in environments where the vectors may have to be controlled. So if you're going to control salt marshes, you have a salt marsh harvest mouse or some other species there, and State Fish and Game is concerned with that animal. Or it might be the kit fox in the Central Valley that they're concerned about. So we must have a very close liaison with State Fish and Game, because we have to work with them in setting up the control procedures that will be used in the event of an epidemic.



The Food and Agricultural Administration and other agencies of the state have to be represented on that committee, as they may control licensing of the insecticides, determine the doses of insecticides that can be used on certain crops, and things of that type. The newly organized State Environmental Protection Agency, the Association of Local Health Officers, Directors of Environmental Health Programs, and the California Mosquito and Vector Control Association all have representation on the Vector Control Advisory Committee.

Some of these appointments duplicate representation we had before, but some of them are completely different. I would say these advisory groups have become less specifically oriented to sciences of certain types and more generally oriented to the public and legal aspects. It's a multiagency representation of the many types of interests that exist in the state. If the members of the committee agree on something, it's much harder for people to object to it.

In contrast, when an advisory group goes to visit Yale University, the National Institutes of Health, or the Centers for Disease Control, it's almost all scientists, almost without exception. The only exception I've run into is the Advisory Council to the National Institute of Allergy and Infectious Diseases, which includes lay persons who may know little or nothing about diseases.

I remember when I was on a subcommittee of that advisory council that was dealing with tropical and parasitic diseases. I don't know how I wound up there, as that's not really my area of specialization. Nevertheless, there was a delightful lady from Pennsylvania on that council and subcommittee who was a very close friend of Senator Fogarty. She was there because it was thought to be important to have a lay person represent the public on the council. She was very wide-eyed about everything, because she'd never heard of all these horrible diseases in the tropics. I wound up having to be her interpreter, to tell her about each disease. We also had an economist who was a second lay person on the committee from the University of North Carolina. Again, he knew nothing whatsoever about diseases. He couldn't understand why everybody wasn't studying the economic impact of each disease, because he wanted to talk about economics and there was hardly ever a grant came through that did so. Incidentally, I thought he probably was right; economic studies are needed.

Lay people on these committees could be very interesting. However, when you came down to having to make a hard decision on what projects would be supported and there were only a couple of scientists and a couple of lay people on the committee, it took



time. Sometimes it was hard to come to a decision because you had to educate two members of the committee before you could come to a decision. But it made the meetings interesting.

Hughes: Do you have a preference as to the type of committee?

Reeves: I think you learn more yourself if it's purely a scientific group, because that way you learn from other scientists. On the other hand, it is also interesting to find out that a lay person's knowledge may be way off, and they may be biased in their opinion until they get educated to the facts. That gives you a completely different perspective on how important it is to communicate such information to the public. So I think both are important, because a lay person gets very bored listening to a purely scientific discussion about DNA, RNA, or other molecular-level biology. As a matter of fact, it's not too interesting to me. Whereas field biology is very exciting. Of course, I'm not biased at all.

### The Australian Connection

Hughes: I have the feeling that we omitted in our earlier discussions what I might call your "Australian Connection." Am I right?

Reeves: That's probably true. I believe I did mention my first visit to Australia in 1951-52. There had been an epidemic of encephalitis in Australia in the summer of 1950-51. A virus had been isolated from cases by workers at the Walter and Eliza Hall Institute, and it was named Murray Valley encephalitis virus. However, the vector had not been identified. It was feared the disease would reappear, so Sir MacFarlane Burnet, director of the Walter and Eliza Hall Institute, borrowed me from California to come to Australia and carry out studies to identify the vector.

It was a great experience. The area of study was the Murray Valley, which had a history closely related to the area of southern California where I grew up. The Chaffey brothers, two Canadian engineers, had developed the irrigation and agricultural system around Ontario, California, a few miles from my birthplace in Riverside. They had then moved to Australia and developed the irrigation system for the Murray Valley. They even named the main street of Mildura Euclid Avenue, which is the main street of Ontario. The street was lined with Washington palm trees from Palm Springs, California. The crops grown were identical to those on our ranch in Riverside--Washington navel oranges, Hale peaches, grapes, alfalfa, and so on. I could relate to the farmers' problems and get their cooperation. The climate was as hot as

Kern County, California, and there were plenty of mosquitoes. I felt at home except for the Australian accent.

A small group of us did the field studies: Dr. Josephine Mackerras and Dr. Elizabeth (Pat) Marks, two outstanding entomologists from the Queensland Institute of Medical Research, Brisbane; Nancy Kent, a young entomologist; Dr. Donald McLean, a young physician at the Hall Institute receiving his indoctrination into field research; and myself. There was no epidemic, but we identified the probable vector, *Culex annulirostris*, which was confirmed subsequently. I also made a visit to Brisbane to meet Dr. Ian Mackerras--he was Josephine's husband and the director of the Queensland Institute of Medical Research--and Patricia Lee, a young technician.

Little did I realize that this visit began what later became what you call my "Australian connection." Pat Lee, the young technician, came to Berkeley a few years later to obtain her Ph.D. in medical microbiology and to work in our laboratory. While here she met a graduate student, Kenneth Taylor, who was in the foreign service for the Canadian government. They married, and some years later he was the Canadian ambassador in Iran when the revolution overthrew the reign of the king. The staff of the American consulate escaped to the Canadian consulate, where Ken and his wife, Pat, gave them secret refuge and masterminded their escape and return to the U.S.A. Ken and Pat came back to Berkeley that year to be feted, and Ken received the Alumnus of the Year Award from the chancellor. They also received a medal from the U.S. Congress. I certainly hadn't foreseen this future for my former student from Australia, and am I'm pleased to add that she continues to be an outstanding scientist.

To extend my Australian connection further, I served on the doctoral thesis committees for three Australian scientists, Ian D. Marshall, Ralph L. Doherty, and Brian H. Kay. Australian universities felt it was good to have a person from England or America on such committees. Later Ralph became director of the Queensland Institute for Medical Research, the dean of the Medical School, and now Pro-Vice Chancellor for Health Sciences at the University of Queensland. Ian spent two years with us as a postdoctoral scientist in Berkeley and then joined the faculty of the medical school at the National University of Australia in Canberra, where he was director of the Regional WHO Arbovirus Center for Australia. Brian Kay became a senior staff member at the Queensland Institute and spent a six-month leave with us. Brian is now a leader in arbovirus research and mosquito control.

In 1975, Ralph Doherty invited me to be the first senior scientist visitor to the Queensland Institute, to give lectures



and to review the institute's research programs on cancer, nutrition, insect taxonomy, aborigine health problems, and arbovirology. In 1981, Dr. Chev Kidson, then director of the institute, invited me back to be made a fellow of the institute. I remember that a part of the award statement said I was "the doyen of arbovirus epidemiology." I didn't have the foggiest idea what a doyen was; it might be good or bad. It was a relief when I found a dictionary.

To finish up my Australian connection, I made visits to Canberra and Brisbane in 1982, 1986, and 1989, to be a principal speaker at symposia on arbovirus research in Australia. These meetings were sponsored by the Queensland Institute of Medical Research and the Commonwealth Scientific and Industrial Research Organization (CSIRO). Each visit had as its objective to assist them in developing a national arbovirus surveillance program, which is now in place.

Review of the Commonwealth Scientific and Industrial Research Organization, 1986 and 1988

Hughes: The next and last consultant job to discuss is the several reviews of the Commonwealth Scientific and Industrial Research Organization in Australia.

Reeves: This is an organization that studies a set of diseases in Australia which are purely of veterinary concern. Bluetongue is the principal one that they're studying. It is a disease of sheep and cattle which occurs almost worldwide. Australia has had this infection as a real problem in their cattle and sheep industry, not necessarily because it's been that serious a disease but because the countries that don't have that disease won't allow any animals or animal products to be imported from an infected area. We have the same problem in the U.S.A. Sperm from animal herds that are known to be infected with bluetongue or even from animals that have antibodies can't be exported. These can be very valuable products when they come from genetically unique animals. You can't export because it's suspected that the sperm might be carrying a virus.

Anyway, the CSIRO group that's centered in Brisbane has asked me several times to review their program on bluetongue and other viruses in animal populations. This infection is carried by a *Culicoides* gnat. I don't really know very much about *Culicoides*, although we've studied them in California with reference to some viruses other than bluetongue that we found they were carrying



here. There is an extensive program at Davis on bluetongue and related infections in the veterinary school. The commonwealth group in Australia wanted an outside person to come in who had studied the biology of vectors of diseases and viruses. The purpose was to review what they were doing and to suggest things they could be doing. You had to write a report. The intention might be to convince the minister of agriculture, who was responsible for that program, that they were doing a good job and needed more resources. So you frequently found that, number one, you possibly would be critical of what they were doing but for their benefit; secondly, suggesting they were not doing some approaches that they might be doing--new techniques or whatever it might be; and thirdly, doing an evaluation of how important the activity was and strongly recommending that it be made a larger and more effective program.

Hughes: Has your experience been that you indeed are listened to?

Reeves: Generally, yes. Sometimes it takes some time to realize that you have been heard. Well, usually they don't move any faster than the budget year. That seems like a long delay if you're the interested person. I have found that a year or two years later, some way or other, the value and impact of that report comes to the surface.

On other occasions I have had the experience--and I prefer not to review those in detail--where what you did seems to have been an absolute, complete waste of your time. You don't know why it was suggested that the review and report be done. I don't like those assignments. I'm really not that interested in going into somebody else's backyard to review their politics, personnel, and so on, because I'd rather deal with the science and the disease problem.

But you make a lot of new friends and acquaintances by doing reviews. The main value to me of doing this type of thing is what I've learned. I mean, usually you learn more than they learn from you. I'm sure of that. You learn new techniques, you see the different environments they're working in. You're not there long enough to really do anything yourself. I would enjoy going to the Northern Territory of Australia and spending a few months there in the field, really working with these people, but that's not what they ask you to do. I have been flown up to northern Australia to see the area from the air so as to understand what sort of terrain they're working in, but you don't have time to go on field trips and work. It isn't because they're paying you so much money that they can't afford it; you don't or they don't want to spend the time.

So it's not the same as going down to the Murray Valley, as I did in the 1952 period, to investigate an epidemic, actually being out in the field and dealing with new mosquito vectors and problems that you haven't dealt with before. That's a straight-out research problem. It's quite different from getting a bird's eye view of what somebody is doing and saying, "You're doing it right," or "You're doing it wrong," and "Have you tried this?" or "Haven't you tried that?" Or "Why are you doing this?" and "Why aren't you doing that?" It's a different ball game, because if you're honest you know you're really not an instant expert. A real plus in these visits are the new and valuable friends you make with top-notch scientists like Harry Standfast and Toby St. George at CSIRO.

This is really one of the reasons I haven't taken many of the opportunities I've had to go on international travel as a consultant to WHO, the Pan-American Health Organization, and the AID [Agency for International Development] program of the U.S. Government. The inference is that if you spend a few days in a place and write a big review, you have solved the problems of the world. I don't have the confidence that I can solve those problems. It is hard enough to solve the problems here in California. I'd like to have something I'm closer to and to feel like I might be helping, not pontificating.

With reference to your question, "Are you listened to?"-- there has been an interesting spinoff from my consultant visits to Australia. In 1984 an International Symposium on Bluetongue and Related Arboviruses was held at Asilomar in Pacific Grove, California. This was a truly international meeting attended by over one hundred scientists and program administrators concerned with the worldwide problems posed by these infections in domestic and wild mammals. The meetings lasted four days. I had the most interesting and difficult assignment, which was to prepare a summary of the problems that were identified during the meetings and to include some perspectives on future research that might resolve these problems. I was surprised that they asked me to do this, as I have done no research on this group of diseases. In truth, I felt presumptuous and somewhat foolhardy in doing it. However, I was assured they wanted an outside and unbiased viewpoint. The proceedings<sup>1</sup> were lengthy. I was assured my recommendations were heard and would be followed.

I heard very little about progress in this field in the next five years, and I do not make a serious effort to follow that

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<sup>1</sup>T. Lynwood Barber and R. M. Jochin, 1985. Bluetongue and Related Arboviruses. Alan R. Liss, Inc., New York, pp. 1-746.

literature. There was supposed to be a second meeting within five years, which would have been in 1989, and one was not held. This year I suddenly heard that the second meeting was to be held in Paris in June 1991 and that the organizers wanted me to attend. My immediate response was, "Not if I have to write another review of a four-day meeting." I was assured that was not to be my task; instead they were going to have a series of committees do it. I went to the meetings, listened to developments in the seven years since the Asilomar meetings, and was amazed to learn that most of what I had recommended had been done. I can only assume I was not responsible but had selected projects which it was obvious to everyone had to be done. However, it was a gratifying experience, and perhaps my summary was listened to and used.



VIII THE SCHOOL OF PUBLIC HEALTH, UNIVERSITY OF CALIFORNIA,  
BERKELEY<sup>1</sup>

Hughes: Let's switch to the School of Public Health. I have a question that goes way back, which may be unfair because I know you're not this old--

Reeves: I'm pretty old. [laughter]

Hughes: Do you remember hearing anything about Thomas M. Logan, who was active in the 1870s?

Reeves: Not really. I've heard the name, but I can't recall exactly why I should remember him, except that he was a pioneer in public health.

The School in the 1930s

Hughes: Why don't you begin where you would like to begin with the history of the school?

Reeves: I can begin with the history of the school at the time when I had my first contact with it. I was a student on the campus in the late 1930s. When I became a graduate student in 1938, I soon became very conscious that there was something called a Department of Hygiene in the Life Sciences Building. There was a faculty and a bunch of students there, and something was going on that had to do with public health. When you got into medical entomology, as I

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<sup>1</sup>See Appendix D for a chronology of selected events related to the School of Public Health.

was, you realized that public health encompassed insect-borne diseases and their control.

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Hughes: Who was in and what was the Department of Hygiene?

Reeves: The Department of Hygiene was not a school of public health as we know it today. It was primarily an undergraduate program. Dr. Margaret Beatty was responsible for a public health laboratory curriculum. She was a very competent microbiologist who was working on diphtheria. This was before we really understood how diphtheria was transmitted or that it became a pathogen when it had a phage virus associated with it, so it created a toxin. She was in on the ground floor of that research. She represented the field of medical microbiology and the training of laboratory technicians.

The area of what we then called sanitation trained sanitarians to work in health departments. This curriculum also had an interesting complex of people. There was Mr. Walter Mangold, who later became known as Mr. Sanitarian because of his training program for sanitarians. The objective was to raise the standards for sanitarians to a college level instead of just representing somebody off the street who collected garbage or chased rats. So they actually had an undergraduate curriculum for people to become qualified to be licensed sanitarians who could pass state examinations and be concerned with controlling environmental factors that affect health. They were elevating the level of sanitarians who were working for local health departments.

Hughes: Did any other school have an undergraduate program in public health?

Reeves: No, as far as I know this was unique to California. It still is almost unique to California, except that now it's in the state colleges, which we'll come to shortly.

To go back, the objective of the Public Health Laboratory Microbiology Program also was to train technicians at the bachelor's level to the level where they could take state examinations and meet the regulations that a laboratory technician had to have for qualification. Before then, there were none. A physician or hospital would hire somebody and say, "This is what you do in the lab," and perhaps forget about it until proven otherwise.

Hughes: This was before the time you appeared on the scene?

Reeves: I came to Berkeley when this sort of program was being established in the school. This was in the 1930s. They also started a pre-administration curriculum. Now, these program were in place when the School of Public Health was established in 1943 and became really functional in 1945-46.

The pre-administration curriculum was taught by people like Dr. Frank Kelly, who was the health officer for Berkeley. He was a full-time health officer, but he undertook this as an additional activity. Harold F. Gray, the engineer whom I worked for in the Alameda County Mosquito Abatement District, also was teaching in the sanitation program of the school and in the engineering department on this campus. He was a sanitary engineer. Dr. William Donald from Cowell Hospital was teaching what you needed to know about so-called medical care as a part of the pre-administration curriculum.

We had a Dr. Eschscholzia Lucia. That's the scientific name of the California poppy. I guess her parents had a peculiar sense of humor. She was a biostatistician. All the people coming into the undergraduate curricula had to have some training in biostatistics. Her husband was a professor of medicine at San Francisco.

Hughes: Oh, the famous Dr. [Salvatore P.] Lucia, yes.

Reeves: The infamous Dr. Lucia.

They really had a very broad faculty, and I haven't mentioned them all, but that gives you an idea.

Hughes: Where was the department?

Reeves: It was in the Life Sciences Building, on the second floor. Dr. Richard Bolt was another physician who was on the faculty. He was retired, and he'd been an active, practicing medical missionary in China. He was an obstetrician-pediatrician type. Basically, they were using many people from the public health community; and I haven't named all the people who were there. Martha Parker was the secretary. She was what we would now call the administrative assistant for the Department of Hygiene. In fact, she ran the department.

This program had a second program that was going on at the time when I got involved in 1940. They were giving short-term courses to retread physicians who were the health officers and sanitarians who ran local health departments. In those days there was no requirement these people should have a Master of Public



Health degree. There wasn't a school of public health west of the Mississippi River at this time. So they would bring in sanitarians and health officers from county or city health departments and give them an intensive two-week course, trying to improve their level of training in their various fields.

I first became involved in that department when I started working on mosquitoes and virus diseases and because of my acquaintance with some of the faculty. I would be asked to do the impossible--to come in and give a quick, two-hour summary of the mosquito-borne diseases of the world, and that was to bring them up to date.

Hughes: Was this the graduate curriculum in public health?

Reeves: Some of this became a graduate curriculum, but it was a very informal curriculum. They even gave the doctor of public health degree to some of these people. Frank Kelly, for instance, who was the health officer of Berkeley, was given a doctor of public health degree. I'm not sure you could call it an accredited degree by the American Public Health Association in the same sense it would be today. Dr. Harold Gray, who was the engineer, got what he called a graduate degree in public health, but it never was a formalized degree. But later it was enough for him to be made the acting health officer of California for two years, which was the only time an engineer ever was the acting health officer for California.

### The California State Health Department Laboratories

Reeves: The State Health Department medical microbiology laboratories at that time also were on the second floor in the Life Sciences Building. The State Health Department actually occupied a whole suite of rooms at the west end of the second floor of the Life Sciences Building. When Dr. Malcolm Merrill was recruited from Massachusetts in 1945 to be the head of the laboratory unit, he was housed there and took advantage of the relationship to the new School of Public Health. They used him in the teaching program in microbiology, but that's also when he started doing a doctoral degree in public health under the old regime, and it carried over after the School of Public Health was formalized in 1945.

Dr. Alcore Brown and Dr. Howard Bodily, who were very competent microbiologists, also were there. Dr. [Edwin H.] Lennette was recruited into the same group in 1946. So at that time there was a real strength in microbiology at Berkeley, plus

right down the hall was the bacteriology department. Dr. Meyer and Dr. Krueger were heading that up, and people like Sanford Elberg also were there.

There were three groups in microbiology in that one building and on the same floor. Almost all of these people were in what I would call applied microbiology. Molecular biology hadn't been invented yet, so these people were working on how to isolate bacteria, rickettsiae, viruses, or whatever from a sick animal or a sick person, how to do a diagnosis, and so on. It was really the days of culturing organisms and establishing diagnoses.

### Lobbying for a School of Public Health

Reeves: Dr. K. F. Meyer was the head of the bacteriology department and director of the Hooper Foundation in San Francisco. He was very strongly supportive of the Department of Hygiene and its importance as a basis for public health development in the West. This was the seedbed that got things organized in 1943 to begin to successfully lobby in Sacramento for the establishment of a school of public health in Berkeley. It would be the first school of public health west of the Mississippi River, and this was a big area. They figured there'd be a big demand for training in such a school by people from all the western states.

Hughes: There hadn't been murmurings earlier about a school of public health?

Reeves: This really was the beginning of an action program to organize one, and you have to give Dr. K. F. Meyer the major credit for operating behind the scenes to achieve it. On the other hand, he was such a clever politician that he knew he shouldn't be the principal spokesman in Sacramento. That's when he got Dr. William Shepard, who was the vice president of one of the big life insurance companies in the West, to be the head of the lobbying group. I can't remember which life insurance company.

Hughes: It was Metropolitan Life.

Reeves: He got Shepard to head up this action program, because it was obvious he'd have nothing particular to gain himself. I mean, he wasn't going to be head of the school or anything like that.

Dr. Larry Arnstein, who was a private citizen from San Francisco, fancied being called Mr. Public Health because of his

interest in lobbying for public health action programs.<sup>1</sup> He owned some merchandise type of business, I think, in San Francisco. He was a very strong spokesman in Sacramento. Dr. Milton Rose, who was the director of the western branch of the American Red Cross, and Dr. Ed Shaw, who was a leading pediatrician from San Francisco Medical Center, were very much involved in lobbying and also dealing very closely with Governor Earl Warren, who was really a friend of public health.

Hughes: Ford Higby was another lobbyist.

Reeves: That name doesn't ring a bell for me.

Hughes: He was the secretary of the California Tuberculosis Association. Another name that I have is George Kress.

Reeves: Again, I'm sorry; I never knew him. Dr. W. S. Harrison was involved to some extent. I don't recall his first name. He was the regional representative for the U.S. Public Health Service in their San Francisco office. He had to be sort of careful of not getting too politically involved, but he was very supportive of the whole move.

In 1943 they got legislation passed in the state legislature and signed immediately by Governor Warren to form what amounted to, technically and administratively, a statewide school of public health. It was the School of Public Health, Berkeley, Los Angeles, and San Francisco. The legislation also made very sure that it specified there would be two departments, one in Berkeley and one in UCLA, because they anticipated there would be a future need for a school at UCLA.

Hughes: But no department in San Francisco?

Reeves: It was assumed at that time that any public health curriculum at San Francisco would be taken care of from the Berkeley end. It never worked out that way.

Hughes: You mean there never was a public health interest at UCSF?

Reeves: No, I didn't say that. There was not an interest in having public health become a prominent part of the curriculum on that campus. It didn't mean they were excluding it, but it wasn't a major

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<sup>1</sup>See Lawrence Arnstein's oral history, Community Service in California Public Health and Social Welfare, Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1964.



concern, as were medicine, or pharmacology, dentistry, and nursing. Now, a number of us were involved in teaching epidemiology at San Francisco. Even after we moved to Berkeley, we were still going over there and giving lectures.

Hughes: Dr. Meyer didn't have any particular interest in a public health program at UCSF?

Reeves: No.

Hughes: Do you know why?

Reeves: I think he thought that politically and academically it was a lot better to have it be in Berkeley where it wasn't under the aegis of San Francisco. I'll come back to that later when we discuss the attempt made in 1967 to get the school moved to San Francisco Chancellor Heyns and I blocked it.

By 1944 they knew they wanted to go ahead and develop a school in Berkeley; they had a budget. They looked around to see who was going to organize this, and they felt they should bring somebody in from the outside, that there really was no one on the present faculty of the school who they felt likely would be the future dean. It was not saying anything against the people who were here; it was just a matter that they didn't feel that was the way to go.

At this time the Berkeley administration also added Dr. Hammon and myself to the faculty. We were appointed as lecturers, as we were beginning to be involved in teaching epidemiology at Berkeley. They had nobody on the faculty at that time who was into epidemiology. They felt that the people covering microbiology, environmental health, biostatistics, and pre-administration could handle those basic fields.

#### The Walter Brown Deanship, 1944-1946

Reeves: So they brought in Dr. Walter Brown as acting dean, and frankly, I'd never heard too much about him before. He was a physician from Stanford. I don't know a great deal about his background, except he was a very fine gentleman, an older person. He didn't have any stake in this himself, because he had already retired from Stanford and had no interest in becoming the permanent dean. But he knew public health, and he was interested in having the school become an effective operation.

Hughes: Was he an interim appointment?

Reeves: Yes, very much so. I don't know the details of this, but I suspect that the president of the university, Robert Gordon Sproul, hand-picked him for whatever reason. In those days Sproul was the president of the university statewide, but he also ran the Berkeley campus. There was no fancy business of a big faculty committee, advertisements, a nationwide search, and so on when a dean was to be chosen. Sproul asked questions and found someone whom he thought ought to be the person, pointed a finger at him, and said, "You're it." The person usually found it very difficult to argue with him about it. He was a sort of terrifying individual--well, not terrifying but very imposing.

Walter Brown was brought in, and he immediately was able to get things really started. Jessie Bierman<sup>1</sup> in maternal and child health was one of the first people brought in. Brown didn't do a lot of recruiting, but he brought in some key people. He made the decision that he wanted Hammon to be the professor of epidemiology, and other people were added slowly to the faculty.

In 1945 they had the first students in the new graduate program. I was one, and I took the biostatistics course because I wanted to get an introduction to that subject. I didn't have any real interest in a degree in public health at that time. There were four students in this first graduate course that was given in biostatistics. Dr. Malcolm Merrill was one. He came from down the hall, where he was the director of the State Health Department laboratories. Howard Bodily came with him; he was the assistant director of the laboratory. They both wanted more biostatistics. There also was a Major Hayworth, who was in the army. I never will remember his first name. He was in the army in this area, so some way or other he got into the class. So there were four of us in the first class.

It was a really bad experience for Dr. Lucia, because she'd only been dealing with undergraduate students. Her way of approaching students was that if they didn't understand it the first time, yell at them a little louder and they'd understand it. It was sort of like if someone doesn't understand English, if you say it loud enough they'll understand you.

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<sup>1</sup>See Dr. Bierman's oral history: Maternal and Child Health in Montana, California, the U.S. Children's Bureau, and WHO, 1926-1967. Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1987.

Well, anyway, she had four of us in the class, she knew us very well, and we knew her very well. At the first session, she rapidly went through explaining something to us and said, "You understand that, don't you?" and immediately one of us said, "No, we don't." She sort of looked at us, and her first reaction was to yell at us. Then she realized she was not going to get anywhere that way, so she became very patient in her teaching of graduate students.

Jerry Edwards, her assistant, was supposed to teach us how to use hand-cranked calculators. We're all mechanical idiots, you know. So she rapidly went through, "You push this button, you turn this thing and so on, and that's the way you run one of these calculators," and we didn't understand. We kept her there for an hour and a half explaining this until we understood it. Well, she'd never dealt with this sort of teaching before [laughs]-- that is, teaching these sort of dumb people who were already established in public health or faculty positions. Youngsters were very fast and intelligent.

Finally, we got to the middle of the semester, and Dr. Lucia said, "I can't do any more for you. You all have your own databanks. Go ahead and do your own analyses of your data and use whatever statistics you have learned." That was the end of the lectures in that course. Thereafter we went to her individually, submitted our project at the end of the semester, and passed the course.

By the next year we began to enroll the first formal classes, and they were small. In 1946 we had a more formal curriculum in epidemiology, biostatistics, and so on, and were beginning to get additional students. These were primarily people who were returning from the war. In some of those first classes we had people who had either returned from the war or had been a health officer someplace. They realized they just had to get their M.P.H. degree. Dr. William Stiles was in the '46 class. He went on to become a faculty member here for many years and taught the introductory course in public health. In one of those early classes we had Dr. Carlyle Thompson, who went on to be health officer in Montana and then in Utah; and Frank Yoder, who went on to be the health officer for Idaho, and then he went to Illinois as health officer there. We had a lot of people who went on to become state and county health officers.

Hughes: Was the war serving to shift interest within public health?

Reeves: I don't think the war did much to shift interest in public health. We were having this huge population growth in California, and



public health agencies were becoming responsible for increasingly large numbers of people. The realization had come that California was no longer a rural state. We had to have modernized, built-up local and state health agencies, as had been done earlier in Massachusetts and New York.

The State Health Department had grown much more rapidly than most local health departments had. Many of the local health departments had part-time health officers who weren't really dedicated to public health. They had a private practice, and they also worked as the county health officer, so they dealt mainly with immunization or outpatient clinics and things of this type. They weren't really trying to develop an organized health program.

### Post World War II Courses

Reeves: We'd also had the advantage of the short-term courses that had been given for sanitarians and health officers. Lenore (Steve) Goerke was one of the people. I can't remember what health department he was health officer for, but he came to the short-term courses, and he later became the dean of the School of Public Health at UCLA. We had Dwight Bissell, who became the health officer for San Jose. He also joined our faculty and taught the introductory course in communicable diseases for years. We had Art Geib. He was a sanitarian from Kern County, and he later became the manager of the Kern Mosquito Abatement District. I worked with him very closely for years. He'd been here in the short-term sanitarian training program. Dr. Ellis Sox was in the short-term course. He came up here as a health officer from the Porterville City Health Department. The City Health Department of Porterville wasn't much. He was doing mostly private practice. Later he became the health officer of San Francisco, and from there he went to be the health officer of Phoenix, Arizona. The Phoenix City Health Department was bigger than the Arizona State Health Department.

Now, the other thing that was happening to the state was that there had been a lot of city health departments. The one in San Francisco was city and county. Berkeley and Oakland were city health departments, and there was the Alameda County Health Department for the remainder of Alameda County. The smaller units were beginning to be amalgamated into larger organizations that took care of the whole county. As a matter of fact, I believe the Berkeley City Health Department was the last city health department in California.

Hughes: Do you remember when it died?

Reeves: I really don't know, maybe in the 1970s. The health officer for Berkeley was also teaching in our school. When I took my course in public health administration for the master of public health degree in 1947, Dr. Frank Kelly was teaching the course. When I took the course in environmental health, Harold Gray was teaching it. When I took the course in epidemiology, I was teaching half of it and Dr. Hammon the other half, and I was taking it for credit at the same time.

Hughes: Really?

Reeves: Yes. I had to take it, as it was a required course for a master of public health degree. Hammon corrected the exams, but I gave half the lectures. It may be hard to believe, but I passed the course.

We didn't have very many students at that time because we were just beginning to gather a little momentum. Well, we still had a lot of undergraduates, because this had become a large program. In fact, an additional area in health education had been added to the undergraduate curriculum. I'd say we usually had almost a hundred undergraduates in the program, who later filled positions in a variety of health agencies. The state and many county civil service laws required formal training at the bachelor's degree level.

Hughes: Were the state universities doing any training?

Reeves: Not really.

In the sixties we finally were forced to close out the undergraduate program. Then all of the undergraduate programs were handed over, lock, stock, and barrel, to what were then called the state colleges.

Hughes: What forced you to close down the programs?

Reeves: They took the budget and undergraduate enrollment privileges away from the school.

Hughes: Because they wanted the undergraduate program to go to the state?

Reeves: They--being the campus administration and regents--wanted the undergraduate programs to be at the state colleges, and they wanted to relieve some of the pressures on the Berkeley and UCLA campuses for undergraduate student enrollment.

Hughes: They wanted the School of Public Health to become solely a graduate program?

Reeves: Yes.

Hughes: Was there resistance to losing that undergraduate program?

Reeves: Yes. A lot of the faculty at Berkeley and UCLA objected to it very strongly, because they believed in it. In addition, we looked on the undergraduate program as a really important stepping-stone for a lot of students to go on in public health or related professional training. A large proportion of students in the undergraduate program went on to medical school or to Ph.D. programs. They were quite competent and loyal to public health.

A total of five areas had developed in the undergraduate program. There was the original sanitary science, public health laboratory, and pre-administration. A health education curriculum was developed by Dr. Edith M. Lindsay, and biostatistics was developed by Drs. Jacob Yerushalmy and William Taylor. So there were five different undergraduate curricula, they all had students in them, and they had competent people handling those curricula. The undergraduate program made a very valuable contribution to public health development in the western United States.

William McDowell Hammon, Acting Dean, 1946

Reeves: Now, in 1945-46, the expansions of faculty that I talked about earlier moved rapidly. In 1946, Dr. Walter Brown didn't want to be acting dean any longer. He'd retired from Stanford, and he wanted out. The first thing we knew about this was when President Sproul called Dr. Hammon in and told him straight out, "Hammon, I want you to be the first full-time dean of the School of Public Health. Here's the contract for you to sign." He handed it to Hammon and talked to him about the school, its future, and so on. He said, "Sign it and send it back, and you're the dean." That's the way deans were selected in those days.

So Hammon came back to San Francisco, came into my office, and said, "Look what I just got," and showed me the contract. "I don't think I want to do this. I don't want to stop doing research and teaching to be a dean. It's very flattering but too early in my career; that isn't my idea of what I want to do." So he called up Sproul and he said, "What proportion of my time can I spend doing research and teaching? What time do I have to spend being the dean?" He said, "You have to be a full-time dean. You



can't do research and teaching. You think about that; sign your contract and bring it back over to me."

Anyway, in a few days Hammon made an appointment with President Sproul. Meanwhile, Sproul had made him acting dean. He couldn't make him dean formally until he had signed the contract. So Hammon went back over to Sproul's office with the unsigned contract in his pocket, and Sproul congratulated him on accepting the appointment and so on and so forth. Hammon politely listened to him for a while, and he finally said, "President Sproul, here's the contract." Sproul said, "Fine." He threw it in his desk drawer and slammed the drawer shut. Hammon said, "Yes, but President Sproul, I haven't signed it." Sproul said in his loud voice, "What do you mean you haven't signed it?" Hammon said, "I don't want to be the dean. I want to go on doing teaching and research." Sproul blustered around about that for a while, and he finally said, "Well, you are the dean until you find someone I think can do the job, and he has signed a contract."

#### The Edward S. Rogers Deanship, 1946-1951##

Reeves: Anyway, Dr. Hammon in some way knew of Dr. Edward S. Rogers, better known as Ned Rogers, and brought him out from New York, where he had been very much involved in research on pneumococcal pneumonia and was actually making vaccines. The antibiotic era had begun to come into being and was putting all the people doing basic research on pneumococcal bacteria pretty much out of business, because the antibiotics were pretty effective at that time. So he'd moved over into public health administration in the New York State Health Department. He came out, and he and Sproul hit it off. So Dr. Rogers was made the first official full-time dean of the school late in '46 or early '47, and Hammon was relieved of those duties. Most people don't know anything about Walter Brown from Stanford being the first acting dean and don't really know anything about Dr. Hammon being the second acting dean of the school. Most people think that the history of the school starts with Dr. Rogers as the dean of the school. Officially he was, but he also continued his teaching and research.

Now, at this time the school was really getting actively involved in recruiting, because it was obvious that in order to have an operating school of public health they were going to have to expand and bring in a number of additional faculty. In addition, World War II was over, and many professional people were mobile. I won't try to go through all the faculty that were brought in, because that would be endless, and I assume somebody's

going to be working this up sometime for a more detailed history of the school.

But amongst the people they brought in during the late 1940s was Dorothy Nyswander, who was already widely recognized nationally as one of the leading people in public health education. She was here continuously after that until she retired at age seventy. She came to our last emeritus professor luncheon, and she's now ninety-one years of age and still very active and very much knows what's going on. Dr. Nyswander developed the graduate curriculum in health education and recruited Drs. William Griffiths and Beryl Roberts to the faculty.

Dr. Jacob Yerushalmy was brought in from New York state in 1947. He was a very innovative and a very applied biostatistician who dealt with very realistic problems in medicine, public health, and biostatistics. As an example of this, he never believed that physicians were doing a good job on anything until proven to his satisfaction. He did big studies showing that the interpretation of X-rays by physicians and radiologists was really a very guessy sort of a game, as frequently they could not agree in their interpretation of an X-ray. In fact, they frequently couldn't even agree with themselves in double-blind studies; they looked at the same X-ray two times and sometimes made two different decisions. Until this got unscrambled, an X-ray could not be a definitive part of a proper diagnosis of tuberculosis and various lung infections, like coccidioidomycosis or histoplasmosis. So he made quite a reputation, which wasn't always popular. He was a devil's advocate on almost anything until it had been proven statistically to stand up to very close review. He recruited additional most competent faculty, including Drs. William Taylor, Chin Long Chiang, and C. E. Palmer.

We recruited Dr. Charles E. Smith and Dr. W. A. Longshore to the epidemiology faculty in 1949. We'd always had a relationship with the School of Engineering. Dr. Bernard Tebbens was brought in full time on our faculty on the engineering side of environmental health, but he also had an appointment in engineering. The school just kept expanding the faculty. Dr. Leon Lewis was recruited to develop a curriculum in occupational health. Recruitment was one of the major activities of the dean at that particular time.

In the late 1940s they started developing an extensive program in hospital administration. Dr. Richard Stull was brought in from Chicago to develop a program in hospital administration, because at that time they were just recognizing that hospital administrators shouldn't necessarily be physicians and that required extensive training in administration. Today, few



physicians hold such positions, and extensive training in business administration is required. He added Keith O. Taylor and Ruth H. Stimson to that staff.

To summarize, Yerushalmy was brought in to be head of biostatistics, Dr. Nyswander was head of health education, and Dr. Bierman was head of the maternal and child health program. Dr. Tebbens was head of environmental health, and this complemented what Walter Mangold and Harry Bliss had been doing as sanitarians. The engineering aspects were supported by Harold Gotas, Bernie Tebbens, and William Oswald.

Hughes: I read of a laboratory which was a joint venture of the School of Public Health and the engineering department.

Reeves: That's at Richmond; it's the Sanitary Engineering and Environmental Health Research Laboratory. It's a joint operation between the School of Engineering and the School of Public Health.

Hughes: That's it.

Reeves: And that's still here. It started primarily as an operation of the School of Engineering, and then increasingly the faculty in environmental health sciences in our school began to use it as a research base. Then some years ago, Dr. Robert Cooper from our environmental health program was made the director. That was the first time they didn't have an engineer as the director. He continued as director until this year, when he retired, and Dr. Robert Spear is now the director.

#### The Navy Biological Laboratory<sup>1</sup>

Reeves: The other agency that became important to the school was the navy research unit here on the campus, which Dr. Albert Krueger had developed during World War II. He was a faculty member in the bacteriology department with Dr. Meyer, and he was primarily interested in respiratory disease problems. When World War II came along, he was in the navy reserve, and he was allowed to develop a research unit here on the campus by recruiting people to form a research unit for the navy, focusing attention on respiratory diseases. That evolved into the Navy Biological Laboratory [NBL], which was reestablished in the postwar period at

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<sup>1</sup>See the oral history with Dr. Sanford Elberg for discussion of the Navy Biological Laboratory.



the Alameda Air Base. It then became an organized research unit of the University of California, Berkeley.

Hughes: Originally it was right here on campus?

Reeves: It was on the campus in the Life Sciences Building and in a quonset hut at the west end of LSB. It was what was left of the bacteriology department during World War II. That's the way the military kept those people here and knew where they were.

Hughes: Was this research on biological warfare?

Reeves: They were involved in part with biological warfare research. Primarily they were involved in trying to develop defensive aspects. I don't think they ever did much on offensive aspects of biological warfare, although there were some activities involved with releasing supposedly harmless bacteria from submarines off the coast to see whether the air drift would bring them into the coastal area. They found they would come in here. They did this to see what would happen if the Japanese subs started releasing things off our coast.

Hughes: That was strictly a wartime activity?

Reeves: Right. But the laboratory continued research after the war, when it was very much involved in coccidioidomycosis research. Dean Smith was very interested in this cause of San Joaquin Valley fever and developed the first skin test antigens, and the first efforts to develop vaccines were done there. The research was primarily related to airborne diseases. For a long time, part of our arbovirus research laboratory was there and was a base for mosquito colonies and studies on vector competence.

Hughes: Why was that?

Reeves: We needed space; we didn't have it on campus. We had good space at the Alameda base. Work was also done there by Dr. K. F. Meyer and associates on development of plague vaccines. Plague is always a problem, as it is widespread in wildlife in the western United States, and it was an actual problem during World War II and during the war in Vietnam, when it occurred in natives and in our troops.

Hughes: How closely was the Navy Biological Laboratory integrated with the School of Public Health?

Reeves: A lot of graduate students did their research there. People like Stewart Madin, who was on the bacteriology department faculty and then transferred over to our school, was the director of NBL for

many years. Dr. Neylan A. Vedros was one of the last directors of the program. We recruited him from NBL as a professor of microbiology. Dr. Elberg was very much involved in research along these same lines at Fort Detrick during World War II, and he had a continuing interest in what went on in the NBL. Whoever was dean of our school had to be very closely acquainted with what was going on. It was one of our major areas of activity, and a lot of money came there for research on a wide variety of different diseases. It was an outlet for research by our faculty and students and provided research space. We also utilized staff from NBL in our teaching program on campus.

Hughes: Did the laboratory become a bone of contention?

Reeves: Yes, it became a real major bone of contention during the period when I was dean, because people claimed it was the seat of biological warfare research for the U.S. Government. That wasn't true. There was no classified research going on there; their research was all open research, and it was well known. Now, people couldn't just march down to the Alameda Air Base and walk into it without permission and go past a sentry gate. It was a U.S. Navy facility. We could and did take concerned persons down there and took them through the place to see activities and review records. Many reviews were done, all of which made it clear that it was not a biological warfare research center. I would say it's never been a point of contention to the degree that the Livermore Lab has been perceived by some to be.

Hughes: Because it wasn't as well known?

Reeves: I don't think so. I don't think that the degree of concern over university involvement in military research was as heavy during the time that NBL was active as it is today.

Anyway, a lot of recruitment was done by the school back in the period of the early 1940s to build up a key faculty and research facilities, and there was a very rapid burgeoning of the number of students who were enrolled in the school. We'd have to go back to some of the records to get those data, but the number of courses and the number of students who were here developed rapidly.

### "Retread" Students

Hughes: What about the type of students?

Reeves: First we went through what I would call the retread period. Almost all of the students in some of the curricula were physicians who were employed in health departments, and they had come back for at least a year to get an M.P.H. degree and get themselves refurbished in what public health was about and newer ideas that were evolving.

The undergraduate program was still going on, with students in all five of the curricula getting bachelor's degrees. We had an M.P.H. degree available for the first time on the West Coast, so people were coming in from all of the states surrounding us. And students when they finished didn't necessarily go back to where they'd come from. They went to many places in the West or on the West Coast, including California, where there were positions available that allowed them to be promoted.

At the same time, a few people had begun coming to obtain the Doctor of Public Health (Dr. P.H.) degree. That was a bonafide, approved degree, accredited by the American Public Health Association. At this time we didn't have any academic degrees in the sense of the Ph.D. program or master of science degree programs, and that didn't happen until 1952.

Hughes: The Doctor of Public Health is not a Ph.D.?

Reeves: No. It's a Dr.P.H.; it's a professional doctoral degree in public health. It has as a prerequisite the master of public health.

The criteria for these professional degrees are established by the Accreditation Board of the American Public Health Association or the Association of Schools of Public Health. They specify what the minimum curriculum to be completed by the students and what sorts of levels they have to attain. Sometimes it's sort of a floating crap game in the sense of what course requirements are included for accreditation, as the focus and designation of those courses are changed almost year by year. It gets very confusing at times for some people. It just depends upon who's on the boards of those organizations and review committees that make the decisions.

So we go through lenient periods when the requirements are, "Let's go back to the good old days where everybody takes reading, writing, and arithmetic." In this case, everybody takes public health administration, epidemiology, biostatistics, environmental health, and public health laboratory. There is no flexibility in curricula.

I believe we now have a reasonably flexible curriculum for the master of public health. The requirements for a doctor of



public health are related to the student doing some very good piece of research which will apply modern scientific techniques to solving health problems. I don't like to use the term "applied research" because that's not necessarily the case, but it also is not a Ph.D. degree. It's not conceived as a Ph.D. that should address development of some brand-new understanding of something science-wise. The Dr. P.H. thesis is related to, "What do we know about science, and is it applicable to solving health problems?" That's the best definition I can give. However, the basic curricula for the two degrees don't really vary that much.

### The Ph.D. Programs

Reeves: It wasn't until 1952 that we finally were able to get Ph.D. curricula and degrees approved through the graduate division for epidemiology and biostatistics. There are now additional Ph.D. degrees in environmental health science and in health services and policy analysis.

Hughes: Why did it take that long?

Reeves: I think, first, we had to build up a critical mass of faculty. Second, we had to convince the Berkeley campus that there was good, basic research being done in those fields. And, third, we had to convince the faculty on this campus that they were science areas and not just some mysterious thing called public health.

I understand the importance of this question, as I was on the Graduate Council for a number of years. I don't consider public health a science; it is a professional field. Subsequently, the Graduate Council did approve a Ph.D. in environmental health sciences, because we were able to convince them it was a science area. But public health had a very hard time getting approved for a Ph.D. in health sciences and policy analysis. It was almost by the grace of God that we didn't have happen here what happened at UCLA. AT UCLA they have a Ph.D. degree in public health but not in any specialty areas. So if a person down there wants a degree that specializes in epidemiology or biostatistics, it's a Ph.D. in public health, and that doesn't give the student the identity that he really wants.

Hughes: I don't quite get the logic of that.

Reeves: It doesn't have to be logical.

Hughes: Well, they must have had some argument for it.

Reeves: They didn't want to proliferate degrees at UCLA, and they figured that was the way to do it.

Hughes: A degree that was comprehensive?

Reeves: It was so comprehensive it covered anything that the school of public health faculty could possibly justify.

Hughes: In granting the Ph.D. at either institution, how did the powers-that-be get around to the idea that public health wasn't a science?

Reeves: I don't know how, because I wasn't a part of that. The obvious thing is that a professional field such as public health depends on many sciences and professions but is not a definable and separate science itself. I know that I had to convince the Graduate Council at Berkeley that epidemiology and biostatistics were sciences, because I was on the Graduate Council at that time and was the spokesman for these two fields of science.

And then we ran into the argument that we already had Jerzy Neyman's group in statistics on the campus. He had endorsed the idea of a separate Ph.D. in biostatistics, but only if it was a joint interdepartmental degree with his departmental group in statistics. So that degree is now a joint degree and an interdepartmental program.

In the early forties there was a Department of Hygiene, and there was a School of Public Health here at Berkeley. The dean of the School of Public Health also was the department chairman for the Berkeley program. This was a funny situation, because a dean has to sign certain things and a department chairman has to sign certain things. Classically, a dean and a department chairman are different people and may not necessarily agree with each other. First the department chairman has to sign things, and then the dean has to sign them. Well, when the dean and the department chair are the same guy, it's sort of hard for that person to wear both hats, and he usually agrees with himself.

#### The School of Public Health, University of California, Los Angeles

Reeves: And then there was a department chairman for the Public Health Department at UCLA. UCLA had a good undergraduate curriculum in sanitation but a very limited graduate program. A. Harry Bliss,

who had been a sanitarian at Berkeley with Walter Mangold, went to UCLA and developed that program.

Hughes: Why just sanitation?

Reeves: There was a demand for training a lot of sanitarians in that region. There was a big vacuum there. The state laws had been passed about accreditation of sanitarians, and they had to have a degree and pass civil service exams, but there was a real shortage of training opportunities for these people.

Hughes: But not in other fields, particularly?

Reeves: Not as much, except for laboratory technicians. However, UCLA very quickly also developed a new curriculum for health officers, so the program just gradually expanded and built up to include health education, biostatistics, tropical medicine and parasitology, microbiology, and so on. The UCLA operation began as early as the late 1940s, but it didn't actually become autonomous until 1960.

Hughes: That makes sense, because by the fall of 1961 the Berkeley School of Public Health bulletin was no longer listing the UCLA courses.

Reeves: Let's see, [Clark] Kerr was the president of the university, and Dr. Wilton Halverson had earlier been named chairman of the department in UCLA. Chuck Smith was the dean and department chairman up here at that time. Smith was very supportive of the UCLA school all along.

Hughes: Was it just a question of size that UCLA remained a part of the school here for those years?

Reeves: It was partly that, and it was partly that there wasn't any strong push by the campus administration at UCLA to have a separate school. I don't remember what the change was in chancellors down there about that time; it may have been that a new chancellor came in at that time. But I think they just finally got big enough to justify it. They were very restless about being called a branch of a statewide school, with the dean in Berkeley. There was a lot of concern on their part about autonomy. There wasn't any concern up here about it, because actually they were being allowed to be very autonomous. At that time Wilton Halverson had retired as the state health officer and had moved to Los Angeles, so he was sort of acting as a dean down there until they brought in Steve Goerke, who at that time was the health officer for Los Angeles County. They brought Steve in as the dean, which he was for some years after that.



Hughes: Did they develop a curriculum that was very much like the one here?

Reeves: Yes, a very similar curriculum. Every curriculum varies with who's teaching the subject, obviously, but the accreditation for a school of public health pretty much dictates what topics are going to be covered and in what way. So I would say that professional degree programs are much less mobile in their content and in their focus of concerns as compared with academic degrees. Academic degrees and courses can change very rapidly over time as the sciences change.

Then in the sixties, and I don't remember the specific year, it was decided that the University of California would not have an undergraduate curriculum in public health anymore. At first there was a lot of question about this decision. Dr. Smith wasn't sure that he wanted to give up having an undergraduate curriculum, and a good part of the faculty agreed; but the word came through that the decision was final. At that time a lot of our resources were shifted into graduate programs. We still had a fair number of undergraduates who hung on for several years before they finished all of their degree requirements.

### Teaching

Hughes: Did you miss teaching the undergraduates?

Reeves: Those of us who had been teaching did, yes, because they were a completely different type of student. It was much easier for me to teach undergraduates than it was graduate students, and the reason was that I was older than the undergraduates. When I met with a graduate class in the 1940s, I was still in my twenties or early thirties, and the age of many students was forty and over; and they practically all had M.D. degrees. Until I learned to cope with them, they were very difficult some days. I had to learn how to be intimidating. [laughter] No, really, I just had to meet them on their own ground and hold my ground. I guess I found it difficult to teach older people just because they were older than I was.

Hughes: What about the difference between somebody in public health and somebody who was in "proper" medicine?

Reeves: If I understand your question correctly, we didn't get a lot of physicians who were interested in "proper" medicine. The physicians who were in public health practice may have come from

internal medicine, pediatrics, obstetrics, or whatever it might be. They usually didn't come from surgery, anaesthesiology, or psychiatry; they came from general medicine practice. The people who go into public health as physicians usually aren't people who want to see sick people every day. Once they recognize that they want to prevent people from becoming sick, they become very frustrated seeing sick people because it is endless. The fact that people are sick means that, some way or other, the medical system may have failed. They like the idea of preventing sickness rather than trying to heal sickness.

Once this concept is accepted, healing sickness is just like putting your finger in a hole in a dike. It's not that people don't need care when they're sick, but basically there's a lot of difference psychologically in thinking, "I'm trying to prevent people from being ill," than "I'm trying to take care of sick people." I think a lot of physicians realize they are not satisfied after the first enthusiasm of taking care of sick people every day wears off. A lot of the young physicians we get are very bright people, and they've decided very shortly after their internship that they don't want to spend the rest of their lives just doing medical practice. They'd rather be in a position where they are planning to prevent disease at a community or higher level.

Hughes: What about in teaching, particularly when you were younger? Was there a stigma concerning the degrees? You had a Ph.D. and a lot of these students had M.D.s.

Reeves: Well, all I can do is speak from my own viewpoint. There was no stigma on my part. We had people who would come in, and we still do, who will belabor one or another profession or academic area because they think that those people have misbehaved in some way or other. They're unethical, they were just there to make money, or whatever their feeling may be on this. Others may feel that some degrees are less demanding or not applicable to public health.

You take good physicians coming into the field of epidemiology, and some of them are still wanting to think as a physician oriented to sick people. That's not what epidemiology is about, because the non-sick people are just as important a part of your study population. They have to recast their whole thought and accept that inapparent infection can be just as important as a sick person as a source of infection in an epidemiological study. The study of reasons why many people in our population escape being sick and determining the reason for this is just as important as learning why some people get sick. But usually the practicing physician is not worrying about this.



So basically epidemiology recasts the way people think about infections, smoking, eating habits, genetics, or whatever the cause of illness may be. A physician wants to talk about what the clinical signs and symptoms of this disease are, the pathology of the disease, and how to treat this or that case, and that's medicine. I have no argument against medicine. I have absolutely none. I want physicians to know all these things. But that usually doesn't teach them anything about the epidemiology of the disease. They may pay lip service and say, "I want to treat the family of a case as a unit" and so on and so forth, for genetic reasons or disease reasons, but very few are really doing it.

Hughes: Isn't there also a conceptual problem of thinking medically in terms of cases rather than thinking of populations?

Reeves: Epidemiologists think of cases as numerators. They're the top of the fraction, the total population is the denominator, and we want to know as much as we can about both sides. The physician who is practicing clinical medicine isn't too concerned about total populations. We can't do anything epidemiologically if we don't have numerators and denominators. I hope that simplifies the differences as far as the concepts and approaches are concerned; they are very different.

In practice today, physicians frequently concern themselves with the environment that people live in, and they're questioning people in their clinical histories concerning occupational and other exposures. An epidemiologist is extremely concerned with both biological and physical agents in the environment that are going to affect health and how to modify them one way or the other to prevent illness. Most physicians are paying lip service when they talk about that. Most physicians don't really care about controlling mosquitoes or making other environmental identifications. That's not their bag, yet it's an example that can be very important if you're dealing with prevention of mosquito-borne and many other diseases.

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Reeves: When the school really began to expand and develop on this campus, the administration was extremely supportive but gave little attention to the question, "Where are you going to put us?" When we came into being as a school on the campus in 1945-46, we had a few teaching rooms, labs, and offices in the Life Sciences Building, but you couldn't possibly put an expanded faculty in there. So the administration was very generous; they put us in Building T-4. The buildings were wooden barracks built during World War II, and many are still standing on the campus. They



were put up during World War II just to add space on the campus for whatever purpose. So we moved into T-4, and we were a very close-knit unit. The T buildings were not very large and provided a few small classrooms and offices but no laboratories. It was like any old wooden army barracks building, limited in beauty and comfort but functional.

Now, this housing did a lot of things. It welded us into a very close-knit unit as far as seeing each other was concerned. We each knew every student in the school; we knew each other very, very well; we knew each day whether we liked each other or didn't like each other [laughter]; we knew any student in the school who was a problem. At the same time, the laboratories were down in the Life Sciences Building or in San Francisco, so they weren't that convenient or near us. Some faculty were in LSB, some were in T-4, and some were scattered elsewhere--at Cowell Hospital or wherever. Basically, we didn't feel like we had a unified school, but we knew we could live with it. We hoped it would be temporary and that termites wouldn't destroy our quarters. The only thing that was really difficult was when a couple of people had a real fight with each other; then you knew you had to work around that for a while.

### Campaigning for Warren Hall

Reeves: In the early 1950s, the first move was made, by Dr. Rogers, to try to get enough money for a School of Public Health building on the campus. The real objective at that time was to get enough money from state sources that he could get matching funds from the federal government, and that's what all the schools of public health in the United States were doing. Harvard, Hopkins, and all those places built big monolith buildings, of which 50 percent or more were funded by the federal government. Dr. Rogers fought, fought, and fought and finally got enough money from the state that if we had matching funds from the federal government, we could have a nice, big building. And then all of a sudden the federal government stopped such support.

Hughes: Is that the Hill-Burton Act that you're talking about?

Reeves: I think so, yes. So by the time we got the money from the state, the federal funding wasn't available to us anymore, and the decision had to be made to go ahead and develop Warren Hall. It was only a fraction of the size that was originally planned. Warren Hall was completed I think in 1955.

Hughes: Nineteen fifty-five.

Reeves: It was the inspiration of Dr. Smith, who by that time was dean, that it ought to be named after Governor Earl Warren, because Earl Warren had been, you might say, the father of the development of the whole public health education program here when he was governor of the state. By this time Warren was chief justice of the U.S. Supreme Court. That was a real move forward, as we had a building and that degree of identity. However, we still had laboratories scattered in the Life Sciences Building, in the Naval Biological Lab, and scattered all over the place; but at least it gave us a degree of unity.

I think it was in the same year that the [California] State Health Department Building went up across the street, and that was a second major development, because we felt the two units were very complementary to each other. Many of the faculty had appointments over there, as I did, and also many of their people had teaching appointments in the school. Basically, we felt that a unity had been established, with the State Health Department on one side of Oxford Street and we on the other. The State Health Department wasn't put there incidentally; it was planned that way.

Hughes: And a combined library.

Reeves: And they combined their library into the School of Public Health Library. Also, the proximity of the two buildings made a lot of jobs available for students. They could do their theses while employed and learn what public health practice was about. It's been a good relationship. It's been hard to preserve it, though, because there have been so many efforts to move all of the state health department activities to Sacramento.

In the mid-1940s the State Health Department was fragmented; it was all over the place. The main headquarters were in San Francisco. It was believed that the administrative offices had to be close to the Public Health Service regional office and the medical schools. However, the laboratories of the department were here in Berkeley. When the State Health Department had to get out of the Life Sciences Building, they had developed space down on Acton and University [streets]. When Dr. Lennette took over, that's where all the laboratory virology program was moved as well as bacteriology. Now, with the new building, all of their laboratory activities could be consolidated into one place along with the administrative offices and other programs.

But anyway, the changes that took place at the school in curricula and in faculty really burgeoned in the 1940s and early 1950s. But as long as Robert Gordon Sproul was here as the

president, selection of new faculty was a pretty simple business. Whatever program had a vacancy gave the dean a pretty good idea of who they thought were the best people available in the United States in that area. The faculty didn't have a lot of committees. Decisions were made in large part by the senior faculty working with the dean and the president, who usually agreed with the dean on any decisions. It was a very rapid and simple process, once you had a vacancy.

Hughes: As far as I know, the School of Public Health in Berkeley is the only one that is not on a medical school campus. Am I right about that?

Reeves: I guess, yes. However, the degree to which the schools of public health are physically closely associated with a medical school is a different question. Even at UCLA, where they're in the same building, I'm not impressed with how close the relationship is between the medical school and the school of public health, and I'm not impressed with how much the students in either the medical school or the school of public health benefit from the other school being there. A student who is on a general campus like Berkeley can benefit by a lot of courses that are on the campus that are never available on a medical school type of campus. As an example, at Harvard, the student in the school of public health, which is closely associated with the medical school, is not close enough to the general campus to really make it easy to benefit from that campus.

Hughes: Do students really take advantage of courses outside the School of Public Health?

Reeves: The more advanced they get in their graduate work, the more they will. There's not much opportunity in the master's program to do so. The undergraduate program used to use many of the general courses a lot more than our students do now, but the students who go into doctoral programs have to use general courses. In part, it's because they're required to include people from other departments in their oral examinations and on thesis committees.

#### Charles Smith, Dean, 1951-1967

Hughes: Don't you think it's the time to say something about Charles Smith?

Reeves: Yes. Charles (Chuck) Smith was recruited from Stanford in 1949.



Hughes: He became dean in 1951, and he was the former head of the Department of Preventive Medicine at Stanford.

Reeves: He was brought here in part to fill a vacancy created in epidemiology when Dr. Hammon went to Pittsburgh in 1949. He also was recruited to be head of the department, which was in anticipation of his probably succeeding Dr. Rogers as the next dean of our school. President Sproul strongly supported recruitment of Dr. Smith. Dr. Rogers was slated by President Sproul to be a vice-president for health and medical affairs or some such important position in the statewide university administration. He would coordinate health and medical affairs for the university. This was Sproul's idea and is what he wanted, as he'd gotten to know Rogers very well and respected his views on public health and medical care. At this particular time, Dean Rogers was also teaching public health administration and was talking a lot about the national medical care programs that were being developed in England and Australia. He felt it was important to present these programs as a part of the general teaching of alternative systems for public health and medical care administration.

So we had a dean, Ned Rogers, who was slated to be in an important position at the statewide level. About this time the politicians at the medical center put their opinion into this appointment with the University of California board of regents and the state legislature. They didn't want anybody in the statewide university administration who talked about medical care on a nationwide basis; it was too revolutionary. Meanwhile, Chuck Smith had been recruited here to be the department chairman and the dean when Dr. Rogers moved up, and for the first time we had a department chairman and a dean who were different people. I had a lot of respect for both of these gentlemen, but they didn't agree on a lot of things as far as public health, medical care, and so on were concerned. They just did not agree on many things.

Hughes: Can you characterize their disagreement?

Reeves: Chuck Smith was a physician's physician. Chuck continued to practice medicine until the day he died, seeing sick people. He had, I think I can say, a very a conservative view of public health, medical care, and everything else. Dr. Rogers was a liberal who was on the other side on these subjects. He was a physician, but he was not a practicing physician. Chuck Smith had very strong opinions and Rogers did also. They were two people who just didn't agree on everything.

That's not a criticism of either one of them, but also at this point it became obvious that Ned Rogers was not going to be

given the statewide position. Here was Chuck Smith as department chairman, and he and Ned Rogers as dean weren't necessarily agreeing on many things. Smith decided he didn't want to continue to be department chairman under a dean who had different views. He had plenty of other opportunities that he could move to, and he was going to move. Rogers said he didn't want him to move. Rogers volunteered to revert back from being dean to being professor of public health and medical care administration and be very happy; let Chuck be dean and department chairman.

So that's the way it was done. It was a very quick transfer of responsibility, and Dr. Rogers returned to being full-time professor of public health administration. He could talk about medical care programs all he wanted, and they knew that he wanted to, and he hoped that the medical people on the San Francisco side didn't object to this; but if they did, that was too bad. He felt that he had to cover the alternative systems in his teaching and research. He wasn't pushing for a nationwide medical care program in the United States, but he at least wanted the people to know about the alternative systems. President Sproul was relieved to have the problem resolved and approved the new arrangement.

Dr. Smith was a very aggressive dean and department head, and he was obviously the one who really engineered the new building here, for which Dr. Rogers had started raising the funds.

Hughes: Did Dr. Smith have good political connections?

Reeves: Dr. Smith was chairman of the State Board of Health for many years. The State Board of Health was a unique organization that Governor Warren had established and given much power as far as health matters were concerned. The board was composed primarily of physicians, but not exclusively, who decided many of the policy items concerning how the State Health Department was going to be operated, and it had considerable power to change the public health code in the state laws. So it was a very powerful unit. Also, Chuck Smith was the sort of person who made it a point to become very well known and respected by people in the state legislature. He didn't hesitate at all to be the private physician and give a lot of advice to chancellors, presidents, and so on about their personal health problems. He was a good physician.

Hughes: So his name was known in Sacramento?

Reeves: Very well known in Sacramento. He was born in Sacramento and had gone to school there. But also he knew his way around politically. Now, Dr. Rogers did, too, but not in the same way. Initially Ned was much more known nationwide, I think, than Chuck



was. He'd come from New York. Later, in 1962, Chuck Smith got the Bronfman Prize from the APHA [American Public Health Association]. This was the highest recognition they could give to a person in public health for their research and administrative accomplishments.

Smith moved very rapidly on recruiting a lot of additional students into the school. He maintained a full personal load of research on the epidemiology of coccididiomycosis the whole time that he was the dean. As a matter of fact, it was a standing joke that every time he sat down, he was sitting down on an extensive sheaf of laboratory reports that he had to read. So he would sit in the lecture I or some visiting lecturer was giving, and he'd periodically reach under his seat and pull out another batch of these lab reports on valley fever patients, sign them, and mail them out. He made sure each one was proper before he'd sign it.

He was in the laboratory at six o'clock every morning reading laboratory tests on coccididiomycosis. He was in the laboratory from five o'clock until seven or eight o'clock every night rechecking the laboratory tests that his staff had done during the day and making sure that the reports were being typed up. He kept a full-time secretary and two or three lab people busy all the time. In addition to that, if he got a telephone call that someone was sick, and had a question about the diagnosis, he went to see them. And if somebody on his faculty or staff or a student was sick, he went to see them. At that time, Cowell was very much an operating hospital, and a student couldn't be up there and be sick without the dean coming to see him to make sure that he was getting proper medical attention.

Hughes: He must have invented a longer day.

Reeves: He spent a long day. I know it because I used to see him at all different hours.

Hughes: Were you close to him?

Reeves: Very close. He came shortly after Hammon left in '49. I had been given the responsibility, which had been Hammon's, for heading the epidemiology program. I didn't have any other full-time faculty person to teach the more medical aspects of the epidemiology course, which differed from the more biological aspects that I was teaching. We had agreed before Chuck came here that he and I would co-teach the epidemiology courses in much the same fashion that Hammon and I had. So we did, until the day he died, and it was a privilege and pleasure to have such an association.



Hughes: Did he pretty much just take Hammon's part of the course? I imagine you had divided up the course, hadn't you?

Reeves: Yes, we had divided it up. But again, his area of expertise was much more complementary to the course than Hammon's and mine had been, because Hammon and I were both doing our research on encephalitis. Dr. Smith was an expert on coccidioidomycosis and other respiratory diseases, which we hadn't been. We both went to each other's lectures any time we were in town, so we knew what the other one was talking about all the time.

Hughes: What was he like as a personality?

Reeves: Nice guy. [laughs]

Hughes: Well, other than that.

Reeves: I don't really know how to express it. He was a very energetic and friendly sort of person. At the same time, he had his own group of contemporaries and friends who were almost all people in medical practice, so he was really a member of the inner circle of the powers-that-be in many aspects of medicine in San Francisco. He'd worked at the San Francisco Medical Center and at Stanford University and was a very respected member of their faculties. He and Dr. Meyer were very close, and Ed Shaw and others of the high-up people in the medical field in San Francisco at the Medical Center were all extremely close social friends of his.

We didn't see a lot of each other socially, but he was also a very social being, more so than any other dean has been. He had frequent dinners at the Faculty Club for faculty, and at the beginning of each year had a dinner to introduce all the new faculty and staff members. At the end of each year there was always a dinner to say goodbye to anybody who was leaving on the faculty or the staff. I continued these functions when I became dean, but they've never been done since then.

I guess the best way for me to express my concern for him is to give you this.<sup>1</sup> [leaves chair to find document] That says how I felt about him. Now, as you would expect, the day that he died was not a very good day in my life. Chuck Smith had been a most effective teacher and practicing epidemiologist at Berkeley for eighteen years and provided outstanding leadership and inspiration

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<sup>1</sup>Publication by the California Public Health Alumni Association, Highlights--Spring 1967. Item "Farewell Chuck," written by Acting Dean William C. Reeves.

to faculty, students, and the public health community in his fifteen years as dean. He was a close personal friend.

### Continuing Education in Public Health

Hughes: Before we go further, let me ask you about a program in the school called Continuing Education in Public Health.

Reeves: It became obvious in the late 1950s and the early 1960s that we really were not meeting all the demands for training that could be carried out at the master's degree level of public health, because there were a lot of people who couldn't come to school. They were out working in health departments in various parts of California, and those people really wanted a master of public health degree. They were quite academically acceptable to come and get it, but they couldn't get away. So an extended program was developed.

Initially the idea was that we might have this be an activity of the Consortium of the Schools of Public Health. Dr. Smith had again been very instrumental in getting the Consortium of the Schools of Public Health established between the Berkeley program, the UCLA program, the School of Public Health of Loma Linda (where there'd been established a Seventh-Day Adventist School), University of Washington, and the University of Hawaii. We had a consortium in which we were trying to share educational problems and training needs in this field.

In California we had developed an extended program for the basic core courses for an M.P.H. degree. These courses were taken into the field and were presented at places in the Central Valley and the general Los Angeles area so that people who were working in health departments could come. It was almost a night school type of presentation. The basic courses in epidemiology, biostatistics, and public health administration were presented, so that people could begin to accumulate a number of credit units needed for a degree at Berkeley.

Hughes: Who was teaching these courses?

Reeves: Our faculty.

Hughes: Did you do that, too?

Reeves: Yes. We were traveling a lot, and we also were taping lectures from a lot of the introductory courses. At that time the campus had a setup for audiovisual recording, so when we would give a

series of lectures--say in an introductory course in epidemiology that everybody had to take--it would be taped. If people couldn't go to these courses, they could take the tapes out. The tapes were used in the continuing education courses, and sometimes they'd even have a telephone network setup so the students in the class could ask questions over the telephone of the person who was giving the lecture. It was a big and costly effort. It was costly not only moneywise but in the time and the energy it took from faculty.

Hughes: Who paid for the program?

Reeves: Darned if I know. I suppose these people paid a tuition fee, and some way or other funds were raised. We didn't get paid a lot of extra money for doing the teaching.

Hughes: This was nighttime, wasn't it?

Reeves: Yes. Travel was involved for students, too, because the people who were taking the courses couldn't take courses during the daytime.

Over a period of a year or two, these people could get a lot of their curriculum out of the way. We had a quarter system then; we'd gone from the semester to the quarter system. They could come to Berkeley later, and within a quarter or so they could complete all the requirements for the degree.

A big effort was made, but it never really flew well. The students in the field were very excited about it and very enthusiastic, but the faculty got the feeling they weren't really achieving the same things by doing it in this fashion. It was too much of a job. At the same time, the university wasn't too happy about it.

Hughes: Why was that?

Reeves: Well, they're never happy about things that are off campus. When I was on Graduate Council, nobody was ever happy about the courses they wanted to give at Livermore Laboratory or someplace else. They're just not happy about that; they fear that the courses are not going to be first rate and meet campus standards. Or if outside people will come in and teach the course, they say again, "It's not going to be our standard." So basically it was a great experiment that died a natural death.

Hughes: Do you remember when it died?



Reeves: I don't really remember. It lasted for about five years, say in the 1960s and had maybe a thousand participants. I don't remember that much detail. I was so busy doing research and so on that the obligations on my time were such that I was quite limited in how much time I could spend doing the extramural courses.

The tapes were an interesting idea, but they never really worked, as far as I was concerned, because in my field a tape became outdated the day it was done. A year or two years after it was made, that tape didn't talk about the latest epidemic; it didn't talk about the latest information. In fact, it rapidly became history rather than a current event. Those tapes were also available on campus to students who couldn't come to a lecture.

One time after a lecture I'd given, I was curious enough to see how it was that I went to hear myself. It was a large class, close to a hundred students, but there were about twenty students who had come in to see the tape. It was early in the term, so they didn't know who I was. I just came in and sat down in the corner. The conversation was very interesting. [laughs] You know, there were sort of snide remarks made when the tape came on and I came on giving the lecture.

About this time, one of the students looked over and recognized me. Then it was interesting to watch because of the problem that student had, namely, "How can I shut these poor guys up before [laughing] they get into really big trouble?" I was having a big time; I was laughing from ear to ear. Some of them never did know I was in the room until the whole thing was over. Then somebody said, "It's sure too bad we couldn't be there to ask some tough questions." I said in a loud voice, "Well, go ahead." [laughter] This student turned around and looked, and it was the student who had been making a lot of snide remarks. It worked out fine. They had good questions, and we had a pretty lively discussion for twenty minutes. The students said, "You should come here every week." I said, "No, thank you."

### Changes in the School's Administrative Structure

Hughes: Do you want to say something about how the administrative structure of the school changed? You mentioned the five divisions but a single department in the very early days, but there were some intermediate structures, too, I believe.

Reeves: Those five divisions were actually undergraduate curricula, as they were based on curricula developed just for undergraduates.

Now, when we developed the graduate program, then the administrative and curriculum structure evolved largely around the fact that somebody was employed to be a professor of a certain topic, and students came because that topic and professor attracted them. There were no such things as any subdivisions that were recognized by the campus. There were academic and professional degrees, but no separation at the M.P.H. level.

Somebody would be brought in, let's say to teach public health nutrition or public health nursing. We had nobody in public health nutrition until Dr. Ruth Huenemann was brought in. She wanted to be sure that the field of public health nutrition as such had some recognition. Her title was Professor of Public Health Nutrition. But within the department, there was no such thing as divisions of epidemiology, biostatistics, nutrition, and so on. And then a person would come in who represented a faculty of one; that later would be two or more. We had a program of public health nutrition which that person was responsible for and had students that would specialize in that area, but no degree in public health nutrition. You see, the degree is a Master of Public Health, and it doesn't say in epidemiology or in this or that. It's an M.P.H. degree. The curricula are what differ. Everybody has to take a basic core of courses and cover that information. Then they take additional courses in their special area of interest.

It was a very peculiar administrative structure; there was a dean, there was a department chairman, and there was no structure beyond that. Formally, we had as many as thirteen programs or divisions in the curriculum. Everyone had to have recognition. The dean might have a meeting of the senior heads of the various programs, and that might be his advisory group, but the university never recognized them. There were no stipends; there were no administrative duties associated with those positions. Administratively it was a nightmare; well, let's just say it was a mess.

As I said, earlier there was a dean, and the dean was also department chairman. Then we had a period with a dean and a department chairman who didn't always agree on everything. When Dr. Smith took over again as the dean, he became dean and department chairman again. Under one side of his fanny he sat on the stuff the dean had to sign, and on the other side the department chairman's. And he ran around with these great big stacks of paper signing things like crazy.

That was the case until I took over as dean. Then we divided-- Well, no, we didn't even do it then. When I took over as dean when Dr. Smith died, I became the dean and department

chairman. It seemed like a lonely place, as Eileen Boston, the administrative assistant, and I made all decisions and did everything. I figured it was time to share the workload.

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Reeves: I went to the faculty and said, "I think it would be a great idea if we had two departments in the school to divide up all the budget preparations, personnel actions, and curriculum development. It would be much better not to have the same person be both dean and department chairman." To my amazement, everybody objected. Nobody seemed to want two departments.

Hughes: Why?

Reeves: They liked it the way it was. No better argument than that was given, except to vaguely say they liked what I was doing.

Hughes: So what did you do?

Reeves: I couldn't do anything. They blocked me. They wouldn't vote for it. It had to be voted on, and if agreed we would have to change the Academic Senate rules and regulations in order to have it. So if the faculty refused to do it, I couldn't do it. The faculty and Academic Senate is all powerful in these sorts of situations. The dean really doesn't get much unless they want it. I learned administrative lesson number one: make them have the idea or at least think it is theirs. So it wasn't until later when I was no longer dean that they decided to have two departments.

Hughes: Nineteen seventy-four--it wasn't that long after you were dean.

Reeves: Well, it was almost right after it. [laughs] They suddenly had a bright idea and decided they wanted two departments. We'll go into that later.

### Administrative Assistants

Hughes: So how did you manage, administratively?

Reeves: How did you manage? You just did it. [laughter] You see, we'd been fortunate all along. Martha Parker was the equivalent of an administrative assistant or management service officer. She was the person who ran the school in the Department of Hygiene in the mid-1940s, and then she ran the School of Public Health when it became a school of public health. Anything that happened in the



school had to have her approval. They're amazing people; they know everything that's going on. They are responsible for student admissions, budget, personnel actions; they're responsible for everything, and they only have a small staff to do it. She was there for years.

When she died, Eileen Boston, who had been on her staff since the mid-1940s, took over. I remember when I first met her; she was the telephone switchboard operator. When I came in, there was this new lady back of the telephone board, and I looked over at her and saw that she was reading a book. So I looked at her and I said, "That's interesting. You're reading The Kinsey Report," which explained all about sex in the 1950s. She was very embarrassed, because she was a very shy sort of a person. I'm glad she didn't consider my question to be harassment. She became the person who ran the School of Public Health when Martha Parker died, and the two of us worked together very well. Every day I came in and said, "What do I sign today and why?"

The third person is there now, and that's Shirley Roach. That shows you how very few people have actually been in that key position in the dean's office all these years.

#### The Reeves Deanship, 1967-1971

##### Acting Dean

Hughes: Let's backtrack to your appointment as dean. Why were you chosen?

Reeves: That's a good question. [laughs] When Chuck Smith was the dean, he ran everything. But there would be times that he couldn't be here, and I was finding increasingly that I was acting dean. I had to sign all the things that had to be signed when he was out.

Hughes: Why were you acting dean?

Reeves: That was before he died. Somebody had to be there to sign things all the time.

Hughes: But why you?

Reeves: Because he asked me to and told the chancellor that I was approved by him to do so. No vote, appointment, or title. He just asked,

"Bill, will you do this while I'm gone?" I said, "Yes." I took it as a compliment and a vote of trust on his part.

Now, as I told you, in 1967 I was in Colombia, up in the Andes, on a special project for the Pan-American Health Organization. The American Committee on Arthropod-borne Viruses that I discussed earlier was functioning extremely well, so someone had the bright idea that there ought to be a South American Committee for Arthropod-borne Viruses that would get together all the workers in Latin America who were interested in this field. They would function like we did, as a very democratic group with subcommittees taking care of problems. They'd have an information exchange, but it would be in Spanish. This was really an outcome of the trip I talked about earlier that Dr. Scherer and I had taken around all of Latin America to review research programs on arboviruses.

So a group of us North, Central, and South Americans were down there pounding this plan out, trying to get these people to agree that they would like to have a South American ACAV. It was very nice. We were up in the Andes, miles from nowhere, in a big hacienda. But we were having a big time. One of the guys from Colombia was collecting orchids like crazy, which stimulated my interest in orchid collecting. As a matter of fact, I got some orchids from him later for my collection as a result of that trip. Unfortunately, there was a telephone there.

But anyway, there we were, and one night the telephone rang and telegrams started arriving, even in this remote place: it was absolutely essential that I come back to Berkeley as fast as possible. Dean Smith had died. There were requests from the chancellor, Roger Heyns, and the faculty, Dr. R. A. Stallones. I was due to leave in four or five days, and I wasn't too sure how urgent it was that I come back that rapidly, because I couldn't do anything about Chuck's death.

But I beat my way back to Berkeley, and found out that the chancellor, with whatever counseling he had from the faculty, had decided that he wanted me to become acting dean. I reluctantly agreed to this, and I told the chancellor and faculty so, because I really didn't want to be dean. I didn't then and I don't now have any ambitions to be an administrator or to be head of things. It doesn't impress me that much or mean that much.

I did agree out of my loyalty to the school, being an alumnus of the school and on the faculty since it started, that I would do this temporarily to be supportive of the system until a new dean was recruited. Continuous faculty pressure was put on me, and pressure from the chancellor's office, to become dean, and I just

said no, I didn't want to be dean. I thought it was that simple, if you just said no.

Hughes: Were they looking for other people?

Reeves: I wasn't privy to what they were doing. Supposedly there was a search committee, because we'd outgrown the business of having the president or chancellor just make a decision. But I still think that [Chancellor] Roger Heyns was making a lot of the decisions here. I don't know. There may have been a faculty committee that was making recommendations. If so, they never told me. I don't know who selected me or how.

### Appointment to the Deanship

Reeves: This went along, and I was acting dean for the remainder of that school year. The following fall I was still acting dean, and there was a search committee supposedly looking for a dean, but they weren't keeping me informed of anything. I hadn't seen the candidates or a list of names. So that fall we were getting ready to open the school, and I told Chancellor Heyns, "I'd really appreciate it if you would come over and talk to the faculty and the student body on the opening of school, because it would be nice to have you." He was relatively new as chancellor. "Tell them how much you like the School of Public Health, and everything will be great." I knew Chuck Smith had been getting along with him very well and thought I was also.

He agreed to come over. We met in the auditorium in Warren Hall, and I introduced him. The place was full of faculty and students. He got up and said, "Well, I agreed I'd come over and talk, but I won't talk until Bill Reeves agrees to be the dean."

Hughes: Just like that.

Reeves: This was the sort of pressure I really wasn't expecting to be put under. The faculty and the student body clapped, and that didn't help me any. It seemed like it was for five minutes; it probably was a minute. I didn't know what the hell to do, so I just finally said, "All right, if that's what it takes to get you to talk, go ahead and talk." [laughter] "But I don't want to be dean, and I have no ambitions to be dean." I just laid it on the line that it wasn't my idea.

That was a very helpful situation, because it gave me a lot of leeway; if everybody knew you didn't want to be dean [laughs],



they'd better be careful what they said to you, or you might just walk off. But it started a very interesting relationship I had with Heyns. In fact, we'd already had a very interesting relationship, because I'd only been here a couple of weeks as acting dean when the phone rang one day, and it was Roger Heyns on the phone.

#### **Attempt to Move the School to San Francisco, September 1967**

Hughes: Was there ever an interest in moving the school to UCSF?

Reeves: Yes. In September 1967, almost as soon as I accepted the deanship, I had a call from Roger. "Bill," he says, "you've got to come to my office at one o'clock today. There's a big meeting in my office, and I have to have you there to represent the School of Public Health, because this meeting is going to have a lot of influence on what happens to the school." I said, "Will you tell me more?" He said, "No, not really. Just be in my office at one o'clock today."

So I went over to his office, and it was obviously set up for a lot of people to come. I said, "What's coming off?" He said, "There's a delegation from San Francisco Medical Center coming over. Chancellor Bill Fleming is bringing over the heads of all his various programs. They want to talk to me about the future of the School of Public Health, and I said I wouldn't do it without you being here."

Well, I knew all these people, because I'd been on the faculty over there for a long time. Bill Fleming had been head of the dental school. He had a lab right downstairs from the Hooper Foundation, close to where my lab was, so I knew him. He had the deans from the medical, dental, pharmacy, and nursing schools in his entourage. They all came in. This was the power structure from San Francisco. Roger Heyns said, "What's your agenda?" They said, "We want to move the School of Public Health to San Francisco." That was the first Roger or I had heard of it. They'd never been that compulsive before, and they were dead serious.

Heyns said, "I prefer not to discuss this with you. I'd rather have Bill Reeves just tell you whether he thinks this is feasible and a good idea. Bill, the floor is yours." I paid my respects to Fleming, because he was a friend of mine. I said, "I'm sorry. I think it would be the worst move that we could make. I can't see anything to be gained by the school moving to

San Francisco." They interrupted me and said, "You can't have a School of Public Health unless it is close to a medical school. It's essential. You can't survive without it." I said, "Well, we've done pretty well so far. We've been over here since 1945. We've been here over twenty years, and we seem to be doing pretty good. I have no evidence that being close to a medical school has been that important to Harvard, Hopkins, or Yale schools of public health.

"I'd like to tell you why I really don't think we should be in San Francisco. We've got academic programs for M.S., M.A., and Ph.D. degrees in our school, and the Board of Regents has already told you you can't have academic programs in San Francisco. You're a professional degree campus. I think it's important for us to have academic degrees, and I think it's important for our students to have contact with a variety of academic departments that you don't have and will not have in San Francisco. We're not a medical school, we're not a dental school, we're not a pharmacy school, we're not a nursing school. We're a school of public health. We're dealing with a different problem than you are." I gave them the big five-dollar lecture, or maybe the twenty-five dollar lecture. You could hear a pin drop.

They said, "We're not satisfied with your reasons." I said to Dr. Fleming, "All right, Bill, I'll be more specific. Where are you going to put us in the pecking order in San Francisco? You've been the dean of the dental school. You're now the chancellor. Where are you going to put us? Are we going to be below pharmacy and nursing, or are we going to be above them? Where are we going to be in the priorities for buildings, budgets, and so on? Where are we going to be? Don't look at me funny; I've been on the San Francisco campus for years. The pecking order is there, whether you like it or not. There's such a thing as a priority area." They didn't want to answer that question, because they didn't know the answer.

After this discussion, I just flat out said no, I didn't think the school should be moved and couldn't think of a good reason why it should; and they couldn't give a good reason why, except that they said they wanted us.

Hughes: Why had they decided that the school should move to San Francisco?

Reeves: I don't know why. Chuck Smith had died, and maybe they saw a chance to grab something. They never said why they wanted it. But Roger closed the door then. He said, "I've sat here, and I've listened to Bill Reeves and I've listened to you, and I can't see any reason why I'd want the School of Public Health to be moved from Berkeley to San Francisco. I don't want to get rid of it.



It's a program I respect on this campus, and I want to keep it here. Until that situation changes, I won't change." End of conversation.

They were all mad or at least unhappy, and they walked out. Roger turned to me and grinned from ear to ear, and he said, "That was interesting. Do you have any idea of the background for this request?" I said, "No, I don't." He said, "I appreciate your coming in and saying what you said."

Hughes: And was that indeed the end of that?

Reeves: They didn't cancel my unpaid faculty appointment or committee involvements on the San Francisco campus. There's never been another word said to me about that meeting. The few times that the powers-that-be on the San Francisco campus have tried to make noises and give us any problem, we point out to them that we're autonomous. Now, we're still in the legislation as the Berkeley-San Francisco School of Public Health. Nobody can ever tell us what this means.

As a result, our budgets and some of our other activities are handled somewhat differently at Berkeley, because we have an intermediary at the president's office who makes us different from any other unit that I know of on this campus. We're responsible in part to Cornelius Hopper, who is the vice president for medical and health affairs in the president's office. Actually, there's little purpose for the school being Berkeley-San Francisco as far as function is concerned.

Hughes: Is that just an historical anomaly?

Reeves: Yes, for practical purposes.

Hughes: Does San Francisco have any functions in public health?

Reeves: It depends on your definition of public health. They use a lot of the language of public health. They'll talk about ecology, they'll talk about epidemiology. And they certainly have courses over there that teach epidemiology in whatever they call the department at the moment. They used to call it the Department of Preventive Medicine. It is not called that anymore, because preventive medicine is an idea nobody liked. They don't want to prevent medicine. Preventive medicine was not a very prestigious sort of name. They now have a Department of Epidemiology and International Health, and we work with them as closely as possible.



UCSF doesn't have anything resembling a curriculum in public health. They do have a program in cultural anthropology which verges on a curriculum. I don't believe they have a degree program over there. There's been some talk about it at times by some of the faculty over there. There are graduates from here on their faculty who have an interest in such things, but it's never really come to be.

There are basic courses in epidemiology and biostatistics at San Francisco that they're responsible for and some in community health problems that they teach for medical students and for other programs on the campus. Then there is a separate group over there that deals with community and family practice. Obviously, they're introducing into the curriculum and the training program a broader base than just a single patient-doctor relationship. They have people like Dr. Philip Lee, who was one of the top people in the United States Public Health Service in public health matters that affected our nation. He, I'm sure, gives seminars or lectures on how you can provide health services to large populations.

I still have a faculty appointment in San Francisco, and I still get all of their Academic Senate announcements, but I haven't been asked to give a lecture there in the last three or four years. I used to go every year and give a couple of lectures to the epidemiology classes. I believe other members of our faculty still lecture in San Francisco.

To return to our original topic, I can't see that a relationship with a school of medicine is essential in any way to a school of public health or to public health practices as they're done in the average community or state at this stage.

### The Free Speech Movement

Hughes: What were some of the issues while you were dean?

Reeves: I guess the primary issue that I had to face while I was dean was whether I could keep this place open and functioning. I must be the unlucky type, because I went into the deanship just about the time that this campus became involved in free speech, People's Park, and other issues. It just seemed like any issue that could come up did, if it could lead to confrontation between a person in an administrative position and the audience that wanted to present something that was different. That's a rather poor way to put it, I'm sure. But I had the feeling during the entire tenure of my deanship that I really was trying to just keep the ship floating--

that is, keep the school open, retain a curriculum for the students, and keep the classrooms available so students could complete their degrees.

As a result of that, I felt, over the years that I was in there, that I was really accomplishing very, very little in the way of innovative academic or community programs, and research was not thriving. It was a very frustrating feeling that I had, because at that time turmoil was the rule of the day.

Hughes: What were some of the student issues with the School of Public Health?

Reeves: The students within the school, initially, as far as I could tell, had few issues, but then they began to listen to all of the campus jive on free speech, that all curricula on the campus were outmoded, classrooms should be taken out of doors or should not be held on campus, and any research supported by the government probably was bad. I mean, all of these things seemed to become issues. Some of the student leaders and faculty within the school were saying to me that anything they did was an attempt to protect the school from being attacked from the outside. I didn't necessarily agree with that as their objective.

It wasn't a game, because it was too serious to be called a game. Demands were being made to revolutionize the curriculum in the School of Public Health. People said, "We're going to completely change it." I say, "To what? We have to be accredited by the American Public Health Association. We have students who came here to take what we're offering, not to do something revolutionary. They want to have classes held in a regular fashion in classrooms and not have them taken off campus. I'm as concerned with those people as I am with the people making demands and attempting to intimidate those who disagree."

I allowed them and even assisted the "activists" to have meetings to express their opinions about things. I went to all those meetings, and I did not take positions at those meetings. I just listened. I happened to have grown up in a society where if somebody wanted to tell you that his plans are to beat up on you or to do anything else you disagreed with, you'd listen to them, because it's very helpful to know. I listened to them, but I didn't take any position.

The result was that I didn't make any major moves, because I knew that I was the one responsible and they weren't. I hoped I was the one who was still going to be here when the present students and outside activists were gone. The faculty who were siding with them had their privilege to express their opinions. I

didn't argue about that; it's a democracy. But I didn't have to agree with them, necessarily. So I refused to make any major moves during that period of turmoil. A lot of people got very confused by that, but I did it on purpose because I felt it was my responsibility.

Hughes: Do you mean a lot of people in the faculty?

Reeves: Some of the people in the faculty. It was very hard to know what the majority of individuals really wanted, because a lot of people were intimidated at this stage, and others were making noise but for a variety of reasons.

Now, basically, that doesn't mean that I was against any changes. I wasn't against change at all. I was against revolution, especially if it would close the place "if they didn't get their way." I was against that. I think that a lot of forward moves were made on campus and in our school as far as minority needs and admissions were concerned. I'd never been very conscious in our faculty that we had any problems as far as employment of women was concerned. After all, we had recruited Dorothy Nyswander as the head of health education. We'd recruited Jessie Bierman as the head of maternal and child health. We'd recruited Ruth Huenemann as head of public health nutrition. Beryl Roberts was one of the early faculty members we brought into health education. We probably had as high or higher a ratio of females to males in our faculty than any other school or department on campus. I couldn't understand some of the noise about fairness of employment. We were constantly attempting to find effective minority people and women whom we could recruit to our faculty.

Hughes: Even before all this broke?

Reeves: Yes. And while it was going on. It seemed like every time we found a really good minority candidate, somebody else could pay them twice as much money as we could as a state university, just to get him because such people were few and far between.

#### Goals as Dean

Hughes: Did you start the deanship with some ideas of things you wanted to change?

Reeves: I didn't start the deanship with anything in mind. I didn't want to be dean, I hadn't planned this, and I had no ambition to do it.



I grant that this is a very poor thing for me to say, but frankly, no, I wasn't a person who was trying to develop a new career in an administrative position. I felt that I was completely untrained as far as administration was concerned. I knew that I had not been trained at all to be a teacher. I'd never had a course in teaching or education. I was trained as a scientist, and I was very happy being a scientist doing research and teaching in my area of special interest.

I'll be candid about it. In my new position as dean I had to do a crash course in learning what in heaven's name maternal and child health, public health nutrition, and other areas were, because I had not been paying an unusual amount of attention to them. I recognized who the faculty were and what they were doing, but I was not that concerned with reading every research grant they wrote and signing an approval of it or reading their research papers. So I was doing a quick refurbishing, establishing a veneer of knowledge in a lot of areas.

Public health nursing: I knew what a nurse was, and I knew what the public health nurse did, because I'd worked in health departments. As a matter of fact, a lot of people on our faculty were teaching these things and had never been in a health department. I'd worked in a health department every summer in Kern County. I knew all the public health nurses, I knew all the sanitarians, I knew the medical care people who ran clinics, I knew the maternal and child health people. I knew all those people and others because I needed to know them; I needed to work with them.

Hughes: How were you getting this information on faculty research and curricula?

Reeves: I had to read all the papers they'd written. I had to go over all their promotions and handle them. We didn't have a department chairman I could turn to. If somebody on our faculty had to have a promotion, I had to review all the paperwork. I had to do all the original writing to send a promotion forward. You learn an awful lot about your faculty in a hurry. I was constantly dealing with some faculty who always had to test the water by coming in and saying, "I have this fantastic offer of a job in Washington." They were only after one thing: they were trying to get a pay raise. I'd say, "That's a good offer. You ought to take it. You'll get a lot more than we can offer you." [laughter] Now, there were people who were an exception to that rule, but somehow there seemed to be darned few of them. I had other things to worry about.

So the answer to your question is not a very considerate answer, but frankly--I won't make any pretense--I had no major agenda. Every time I started to have one, there would be another crisis. There was the accusation that Navy Biological Laboratory was the seat of biological warfare for the U.S. Government, and I had to deal with that problem. The next one was that three students from the school were in jail because they marched in the Free Speech Movement, they had been picked up and were in the hoosegow at Pleasanton (or wherever it is out there in the middle of nowhere), and would not be allowed on campus until there was a court hearing. This would all be happening just before final exams, so they were being blocked from finishing their degrees. I had to find a way to get around these problems. Basically, the deanship was a housekeeping sort of a thing.

#### Hard Dollars for the Faculty

Reeves: Let me give you a few examples. Before I took over as dean, I hadn't paid much attention to the paperwork regarding budgets and personnel because Dr. Smith and Eileen Boston, who was the management service officer at that time, took care of all of that. So when I walked into the job, I didn't know anything about sources of funding. I had always assumed that the faculty members in the tenure track--that is, the assistant through full professors--were all on state money full time, because I knew I was. To my amazement, I found out this was not the case, that a lot of these people were half on hard money from state funding--what we call 19900 money in the university jargon--and half on soft money from the federal government that was coming in for teaching, research grants, or whatever it might be.

I wasn't sure how much longer the federal government was going to be giving money as willingly as they had been for public health training. We even had faculty who were on what were called career awards from the Public Health Service--not career development awards, but career awards. A career award meant that these persons had been selected as outstanding in teaching in public health and had been assured a salary for life or for their tenure at the university. So they didn't have to be paid from state money, as the federal government was paying for it.

Well, I thought, that's very peculiar, because I don't know how the federal government can undertake an obligation for a tenured professor in the university and assure the salary for

life. As governmental regimes change, obligations change. It's an old, known fact that one Congress cannot obligate the next Congress if the next Congress doesn't want to be obligated.

I got very concerned about this and thought that we had a lot of people in the school who were tenured faculty partially or totally on soft money, and we had an increasing number of people in research or other positions who were being employed on federal money who were in irregular faculty positions, but part of their salary was being paid with state money. They were not assistant, associate, or full professors, and were not in a tenure track. So I decided to make sure that all of the tenured or possible tenured faculty--assistant, associate, and full professors--would be 100 percent on university money.

A lot of people objected strongly, because it meant some people had to be dismissed or funds found from outside sources, because they were not in the tenure line, but I went ahead and did it. It was only a few years after that when the federal funding began to collapse. It posed a lot of problems for us, because people who were on federal money had to be released when their funding was cut off, and they felt that they should have a first choice for any position in the school that became vacant that was on state money. Some of them either didn't meet qualifications for such consideration or for some other reason were not going to get such appointments, and it led to some appeals to the campus committee on tenure and privileges. But at least the regular faculty never suffered from having their jobs cut back to part time, because basically, the university regulation says you can't tell a tenured faculty member that now he is half time, at least not without some difficulty. I felt that was one of the major changes I was able to accomplish when I was the dean.

### Student Demonstrations

Reeves: One day I was sitting in the dean's office. Oxford Avenue was a solid line of National Guards, bayonets attached. Nobody could get on the campus, nobody could get off the campus. In Governor Reagan's judgment, they shouldn't and wouldn't. There was a group of students out there, or people--I shouldn't say students because I don't know who they were--that was taunting the National Guard and throwing things at them. This was going on right outside the windows of the dean's office, on the lawn. The telephone rang,



and it was a colonel from the U.S. Army who was in Washington and had been head of preventive medicine there. He had just gotten his orders to go to Vietnam to solve disease problems in that area.

"Bill," he says, "I'm going to Vietnam tomorrow, and I have some questions about Japanese B encephalitis, dengue fever, and things like that which I'd like to ask you before I go. Is this a convenient time for me to talk to you?" I was sitting in my office by myself--you know, the captain of the ship has to stay there and face the problem--looking at this mob, and I said, "Sure, go ahead. I haven't anything else to do right now." [laughter]

So he started asking me these questions, and I started answering them. About this time the commander of the National Guard lost his cool, so he had a couple of gas grenades tossed up on the lawn amidst these people. You could hear it very loud and clear; I mean, I have nothing but glass between me and them. These people started screaming, yelling, and running around. The colonel on the phone said, "What was that?" I said, "What did it sound like?" He said, "It sounded like grenades going off." I said, "Right." "Wait a minute," he said. "What do you mean?" I said, "The National Guard just flipped a couple of gas grenades up on the lawn out here, and you heard them go off." "Man," he says, "you don't want to talk to me." I said, "What else can I do? I can't go anywhere."

Meanwhile, this crowd broke, ran through the library entrance, and came out on the other side. They went from the north side of Warren Hall across to the south side. When they came out on the other side this helicopter descended on them and regassed them. It sounded like the helicopter was in the building. He said, "What's that?" I said, "What's it sound like?" He said, "Sounds like a chopper!" I said, "Right." He said, "Well, what happened?" I told him. "Look," he said, "I'll call you when I get back from Vietnam. [laughter] I think I'll be happy I'm in Vietnam and not in Berkeley." That's just about the way I felt. I hung up and vacated the building, going out last, as any good captain should. When I came home, my wife said, "How were things at the office today?" I said, "Bad."

Hughes: You didn't like it.

Reeves: I didn't like it. I also didn't like it the day there was a crowd outside of Warren Hall yelling, "We want Reeves." I had the head of the campus police and the vice chancellor for student affairs sitting in my office. We were sitting there listening to this. I'd told them there was going to be this march, because I'd gotten

a leaflet from my head animal caretaker, whose roommate had printed the leaflet. They were marching about NBL [Naval Biological Laboratory], which they proclaimed was the seat of biological warfare for the U.S. Government, and the School of Public Health, particularly the dean, was responsible for it.

We must have had a hundred riot police in the building. I had told the campus chief of police that morning, "Now, look, if that mob gets into this building and starts breaking into Revcos and throwing viral cultures up and down the corridors, we're going to have biological warfare at its peak, because there are going to be a lot of people getting sick. Every virus that I've ever collected is in those freezers, whether it's from Japan, Okinawa, or Kern County--wherever I have collected viruses. They're all there. They infect and kill people." They didn't take me seriously until I told them that, so they had a hundred riot police in the building and a couple hundred outside on standby.

To make a long story short, the chief of police said, "Is there some reason you're here?" I said, "I thought I was supposed to be." He said, "If you weren't here, it would be very simple. I'd just go outside and I'd say, 'Come on. A couple of you representatives come in here. The dean's not here. Why are you standing out here shouting his name? He isn't here. Hasn't been here.' I'll even lie to them if needs be." I said, "How are you going to get me out of here?" He said, "I'll take you out the back door. They don't even know there is a back door. Where do you want to go?" I said, "I want to go to the chancellor's office so I can tell him what's going on over here."

So he took me out the back, put me in a police car, and took me over to the chancellor's office. The chancellor was having a meeting of all the deans to talk about the latest crisis--Reeves is surrounded in his building--and I walked in. "How did you get out?" "The police brought me over."

The classic was when the office of the dean of Letters and Science was invaded, and there was a sit-in in his office which was going on for quite a while. He asked the students if he could use his telephone. He called the chancellor's office and said, "We've got a little problem over here. I've got about fifty students captive in my office. What do you want me to do with them?" [laughter] Anyway, you had to keep your sense of humor.

Hughes: It wasn't too humorous at times.

Reeves: Well, when they threw bricks through the chancellor's office windows, that was no fun. When I came to Warren Hall one morning

and found liquid solder had been put in the locks to the building, I wasn't pleased. I could go on but will not.

### Dealing with Minorities

Hughes: How soon did minority enrollment go up? Was there a direct cause-and-effect relationship?

Reeves: I really can't give you any statistics because I wasn't keeping statistics on that. You've got to get that from the dean's office if you really want to plug in statistics. I was conscious of the fact that there were a lot of people very concerned about the problem, and I was, too. When we had applications for the programs I was mainly involved in, we took the best qualified students, and we hadn't been paying too much attention to their age, sex, or racial background. That hadn't been my priority at that time. It was forced upon us to a great extent after that, as we were being held accountable by very vocal groups and our personal views. Meanwhile, we were developing the first program in the whole United States to train American Indians in public health, and it's still a major program in our school.

Hughes: Say something about that program, would you?

Reeves: I can't, because I wasn't responsible for it.

Hughes: You're talking about the Window Rock Navajo Project?

Reeves: Yes, in part. It started off with Bill Griffiths, Beryl Rogers, and others in public health education having field research programs in the Southwest region on the reservations. But I wasn't immediately involved in that. They had research programs on health problems of the Indians at Window Rock and other areas in the Southwest. As a result, they began to bring in people from the various tribes who were qualified for admission into the university and into their degree programs. I can't tell you when the program started or details about the federal support for the program, but it grew very rapidly.

Hughes: Nineteen fifty-five is the date I have.

Reeves: That wasn't a major item on my agenda when I was dean. But it was a major item on my agenda when the first black student we had, from the School of Optometry, was in the hoosegow because he'd gotten caught up in the Free Speech Movement. I became very



protective of him and getting him out to make sure he got through the school year.

Hughes: He was in optometry?

Reeves: He was a graduate of optometry. Dr. Smith would never admit a person from optometry into the school.

Hughes: Why?

Reeves: Because they weren't physicians.

Hughes: You didn't feel that way?

Reeves: I thought it would be very good if some of these people could do the Master of Public Health degree and take this back into their community practices. We had quite an argument about this student before he was admitted into the epidemiology program.

Hughes: Did it work out well?

Reeves: Well, he completed his degree and disappeared into Watts. I didn't know where he was until he showed up in my office a few years ago. He is now in international health, doing program developments for the USAID program in Latin America.

Hughes: In general, did it work well to take optometrists?

Reeves: No, we haven't taken a lot of optometrists. But there was a druggist who applied to the school. Nobody wanted to take him in. I said, "I think he'd be a great investment, because he's a darned good lobbyist in Sacramento." We took him in, and he became an even better community lobbyist. He's still in practice someplace over in Marin County.

Hughes: Getting back to the minority issue, in 1971, which was the final year of your deanship, the American Indian graduate program began.

Reeves: I was supportive of the initiation of the program, but I wasn't the person who was out front on that. It was a program that had enough initiative from our faculty that I didn't have to do it. I was not unsupportive of any of these initiatives, but I wasn't making them major issues that I was spending all my daylight hours on.

I don't think the actions that were taking place on the campus had a lot of influence on the new programs. I think we were at that point where public health programs generally were recognized and needed to be a partial answer to many minority

needs. The Watts incident and all other such events led us to do things, as it made us recognize how important it was that the needs of some of these groups be met. I didn't feel that I had a personal competence in these sorts of areas. That's one of the reasons I didn't want to be a dean. I made no bones or excuses about that.

### **Resignation as Dean**

Hughes: Why did you step down in 1971?

Reeves: I couldn't live with myself anymore. I'd learned to dislike too many people. I was tense, and my blood pressure was going up. I wasn't enjoying myself or life at all. I didn't feel I was the person to lead the school into new areas of endeavor. I also felt that the urgent "war of survival" for the school was over.

Hughes: Had you been able to do any research?

Reeves: I had been forced to give up all of my competence to do laboratory work in virology. The whole field had moved completely away from anything I was competent in. I could still think like and work with mosquitoes; I could think like they could, or at least I could outguess them sometimes. Biologically there was no change in the basic outdoor environment and problems it presented, and I wanted to get back to doing that sort of thing and thought I had enough ideas that it was worth doing.

I couldn't see myself spending the rest of my life trying to become a generalist in public health and in administration and a politician for public health. I thought it wasn't fair to anybody. Particularly I didn't like it because some of the efforts I had made had failed. I wanted to have at least two departments and department chairpersons who would take on a lot of the administrative responsibilities and believed that could accelerate the school's movement into new areas.

I had brought people from the academic faculty into the dean's office to be associate deans, and to take some of that load I brought in Warren Winkelstein, Len Syme, and Alberta Parker to be associate deans to handle one or another aspect of things. When I did that, I brought in Academic Senate members. That has not been the general approach that the dean's office has taken since I left. They brought a lot of nonacademic people into those sorts of positions.

Hughes: Is there a reason for that?

Reeves: That was their choice, the way they wanted to go.

Hughes: What is the logic?

Reeves: I have no idea what the logic was. I didn't do it. A thing that's illogical to me may have been very logical to somebody else. I'm not saying this to be derogatory about the people they brought in. I thought that academic people should be brought in in order to have a training ground for them as possible future administrators. That was successful in the sense that one of the persons I brought in became the next dean, and another one became a department chair. I thought faculty would prefer to have academic people making decisions that influenced their research and academic activities rather than non-academic personnel.

Hughes: Warren Winkelstein followed you as dean. Was there any sort of mentorship period?

Reeves: No.

Hughes: You stepped down and he came in?

Reeves: No. I stepped down with no forewarning. I woke up one night, and I just turned to my wife and said, "Are you going to be unhappy if I resign as the dean today?" She said, "I've been waiting for you to say so. I hoped you would." The next day I went to the chancellor's office and found out he also was resigning. [laughter] It's very nice to have a wife and chancellor who understand and frequently agree with you.

No, there was no preparation. I'd had little or no preparation, and Warren really didn't either. He had been associate dean for academic affairs. Actually, he had to be acting dean for a year before they finally stopped going around the rosebush looking through every possible candidate on God's green earth and made their choice. My position when I walked out as the dean was--and I told Warren when he was appointed--"I'll never give you any advice about what you should do as dean. If you ask for advice, I'll give it to you, but I'm not going to look over your shoulder. It's your responsibility." I told Joyce Lashof the same thing when she came in. I think it's the worst thing in the world to have somebody telling another person how to do a job that maybe they didn't do as well as they should have.

Hughes: Did Dr. Winkelstein ever come to you?



Reeves: No, not about the dean's concerns. We're very close friends. We teach together and share in social activities.

### Changes in the Student Body and Its Interests

Hughes: Do you have any comment to make about changes in the student body over the years?

Reeves: Yes, they get a lot younger every year. [laughter]

Hughes: Yes, I notice that, too.

Reeves: Seriously, there have been a lot of changes. As I said, originally we were teaching retreads, so you didn't get students who were going to do a lot of brand-new things. These were mainly older people who were established as health officers or had some other position in health agencies. You were doing your best to bring them up to date on diseases, methodologies of study, control programs, health administration--whatever it might be that you were teaching. You were trying to give them new developments that they could take back and use. But we didn't get a lot of students in the M.P.H. or academic programs who were going to go on to be scientists or people going into academic positions.

You saw the picture that's in my den of the class of 1949 in epidemiology. It was sort of a typical small group. Here is Colonel James H. Gordon, who as soon as he finished his degree with us became head of preventive medicine for the U.S. Army. He was a full colonel in the army and his career was set, so he went back there and held that position.

Here's Dr. Julius Amsejius, who was a young veterinarian. He'd never had a job in public health, but he decided he wanted to get out of practice and go into public health. He became the first public health veterinarian for the state of Arizona, and still lives down there.

Here's Lloyd F. "Dusty" Miller, a physician and commissioned officer in the navy. I don't know what rank he was at that time--probably a full lieutenant or the next step up. He was a medical officer, but he was also a microbiologist, and he came to take the Dr.P.H. with us in epidemiology, because it was the only program

that would allow him to pursue a thesis<sup>1</sup> in doing drug resistance research on plague with Dr. K. F. Meyer. He eventually became head of the medical research command for the U.S. Navy.

Then there's Rosemary Brunetti. She was a physician type who was working for the State Health Department in epidemiology, and when she left here, her husband, who was in the State Fish and Game Commission, was transferred to Sacramento. She went to Sacramento as the assistant health officer for Sacramento-Yolo County.

There's Leon Rosen, and he was the only one who was a young person. He'd been a medical student with us in Kern County in '46-47. He was a person whom you knew was headed on a research path. He might go academic, and he might just stay research. He became one of the leading research people in virology in the National Institute of Allergy and Infectious Diseases. He made a career of studies on communicable diseases, respiratory diseases, diarrhea in orphanages in the Bethesda area, and then became head of the Pacific Research Center for the National Institutes of Health in Honolulu. He spent the rest of his life being purely a research scientist and became a leading authority on mosquito-borne diseases. He has an M.D. degree from San Francisco, an M.P.H. in epidemiology from Berkeley, and a Dr. P.H. degree from Johns Hopkins University.

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Reeves: Finally, there is Dr. Glen Baird, another physician. He didn't want to be a private practicing physician anymore. To my amazement, the next thing I knew after he completed the M.P.H. degree, he'd become head of the preventive medicine and public health program at the University of Alabama Medical School in 1953. He died in that position.

That shows you the variety of students we had, from people who were really mature, positioned people in their forties, down to students who were still in their twenties. Today we rarely see the retread type of person who has already been out practicing public health, because they can't get into those positions without an M.P.H. degree. We get a lot of physicians who don't want to do their private practice anymore. They're bright, and they come to

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<sup>1</sup>L. F. Miller. The Occurrence of Streptomycin and Sulfadiazine Resistant Strains of *Pasteurella pestis* During the Prophylactic and Treatment of Air-borne Plague in Mice. 1954. Dr. P.H. thesis, University of California, Berkeley.

school to get retrained to do another thing, such as epidemiology, occupational health, or maternal and child health.

We get a lot of physicians or people with Ph.D.s, who suddenly decide they want to go into international health or research in a health field, and they need to get the additional training in a field such as epidemiology. They may already have a Ph.D., but they come to get a Master of Public Health and maybe a Dr.P.H. as well, to get into the international or some research activity.

We have a relatively new program in epidemiology and biostatistics, an M.P.H. degree, that attracts people who are right out of bachelor's degree programs. They're excellent students who have had no practical experience whatsoever. We used to look for practical experience in public health as a major requirement for admission to an M.P.H. degree. These students are here for at least two years, they take an M.P.H. degree program in epidemiology and biostatistics concurrently, and then they go every which direction. They take a position in a health department; they use the degree as a way to get into medical or veterinary school; they go on for a Ph.D. in epidemiology or biostatistics. They're bright young people, and they just want to have the tools to go to work at the master's level, get into a health program where they need a biostatistician, or go on to a doctoral degree in a health-related field or medicine or veterinary medicine. I'm talking particularly about the students that I have intimate contact with.

One thing that concerns me is that I have no contact with the students who are in the other department, which includes medical care, health education, public health and medical care, and so on. This is because I don't teach the introductory course in epidemiology anymore. Besides, when you do, the course has 150 students in it. If you try to use a Socratic approach and get them to enter into the discussion, you're wasting your time. In a class that size, that's not a good teaching technique. So I don't have any contact with them, and I don't have any opportunity to try to communicate with them.

It's obvious that there is a huge increase in the number of minority students within the school, but the greater part of this increase is not in the more science-oriented areas, which would be epidemiology, biostatistics, microbiology, and environmental health sciences. The reason is that we don't get many minority applicants who have the necessary undergraduate preparation. We get the occasional American Indian, black, or asiatic, and they do very well.



One black student that I had, Dr. James Ferguson, was a veterinarian whom the army sent as a commissioned officer to get a Ph.D. in epidemiology. He had been a veterinarian who was a Green Beret in Vietnam, where he worked on diseases in guard dogs. An infection was killing a large number of dogs until he found out what was causing it. He came here and did a Ph.D. in epidemiology with us. He went back to Walter Reed Medical Center in a preventive medicine program, from there moved to the National Institutes of Health as secretary for a study section in infectious diseases, and then over to an array of positions in the National Institute on Drug Abuse. I understand that currently he's the new dean for the veterinary school at Tuskegee, from which he was a graduate. He hasn't let the grass grow under his feet, but he's never done any of the research that he was trained to do here. [laughs]

Hughes: Has the purpose of the school changed over the years?

Reeves: I guess it depends on whom you talk to. I don't think the purpose of the school has changed as far as the programs in epidemiology, biostatistics, medical microbiology, and environmental health are concerned. We're still trying to train scientists to work in those fields. The basic methodologies haven't changed that much, but the scientific techniques have. Epidemiology has become more statistically oriented than it was before, and molecular biology has given new techniques to solve problems in many biological field.

I think the diseases that students are interested in have changed dramatically. With the exception of AIDS, for practical purposes there's little interest anymore in the infectious diseases. The public, legislative representatives, and news media, and therefore students, don't consider them to be the major health problems. Students are interested primarily in chronic diseases--cancer, heart disease, genetics, and such things. They're very interested in environmental factors that are going to affect health. For instance, this year in the one class in epidemiology for the M.P.H., there were thirty-some term papers. With the exception of AIDS, there was only one on infectious diseases. The remainder were in chronic diseases or other problems that cause diseases (social or environmental).

In some ways the many studies on AIDS are a little difficult to differentiate from the types of studies that are done on chronic diseases. Generally the studies deal with large population bases and how to teach people to avoid this disease by changes in personal habits rather than by community changes. The main hope seems to be that there will be a vaccine or drug

developed so that they can take care of the ones who have it and hope that it will just go away. I don't think it will.

The student body is much more interested in community action programs than they are in the other ways to solve problems. We have more joint degree programs. For example, the students take a master's degree in the business school along with a master's degree in hospital administration. So they're combining degree programs that broaden their employment possibilities and their training base.

### Foreign Students

Hughes: What about foreign students?

Reeves: When I came on this faculty we had a very large number of international students, because there was a real need to train international students to go back to their countries, not only to work in health programs but to establish schools of public health. I thought our curriculum was a poor base for an international student, because we were here primarily to teach students from the United States. We dealt primarily with problems in the United States because that's the area of expertise of our faculty. Candidly, I knew that I didn't really know how to solve the problems that occur in other countries. I've never done it, and if I haven't ever done something I don't have much confidence in my competence.

Initially we had the idea that we were primarily trying to bring people here who were going to become health program leaders in those countries and who could initiate the establishment of schools of public health to train people there. With this objective, our load of international students would be primarily the people who had risen to leadership positions in those countries and who would benefit by coming here to see what they might do at the upper levels or in the future. That's happened to some extent, though not to the extent that I would hope, because I still think that a large proportion of international students are not really that concerned with returning to their own countries to do what they say they're going to do. The real ambition of many is to come here and stay. At least that's been the case for a very large proportion I've had contact with.

Hughes: Does that disturb you?

Reeves: Yes, it disturbs me. It disturbs me, number one, because I don't think we need them here to fill our public-health positions. It's very difficult for them; frequently they cannot get the sort of job they dreamed of. Frequently their degree training in medicine won't allow them to practice here, or it will require that they do an internship and residency here. So a lot of these people don't wind up that happy. Plus the fact that if we bring them here with the premise that they're going back to benefit their people at home, and those people need them and they don't do it, then to me it's a failure when they don't return home. That's the only time I feel it's almost a failure in our program. A flat-out failure academically is a failure, but if a student is going to do this or that and then doesn't do it, to me that also can be a failure. Not on our part.

I've been very disappointed over the years with the number of students who will go to any length once they've gotten here to not go home. At the same time, I've been very pleased with the accomplishments of others who have been here and with what they've gone back and been able to do. We really had an objective with other schools of public health to train a nucleus of people to go back and establish schools of public health in a mode that would meet the needs of those other countries. There have been a lot of schools established in Latin America and Asia on that premise.

Another problem is when a student wants to take with him the whole curriculum from our school, because that's what he wants to teach back home. I say that's not the way to do it. You want to take the methodologies. But if they ask for our curriculum: "Can I have your exercise on disease X in California to take back home with me?" I say, "No. You shouldn't take that there. Who in the hell cares in Timbuktu what happened in California with disease X? Go back there and develop your own teaching material with the problems and data that you have there." "But I want to take this curriculum. This is what I learned here." Well, in those cases they haven't gotten the objective or knowledge they came here to get.

So that's a problem, and I don't know what to do about it. But I guess I never have developed any pretense of a real philosophy of what things should be in this regard, what they are, and what we should accomplish. I haven't spent a lot of time thinking about that. There are other things I want to think about.



Bond Issues

[Interview 11: June 5, 1991]##

Hughes: Is there any more you wish to say in relation to the School of Public Health?

Reeves: I'm not sure I want to talk about bond issues, but I think it's a topic that deserves discussion.

When Warren Hall, the home of the School of Public Health, was built in 1955, it happened to be a time when there was no federal money available for matching funds to build schools of public health. There had been earlier matching funds that many other schools had taken advantage of, but we didn't have any money at that time to do so.

So the result was that we had a building that was very nice. It was new. It had a lot of space, a lot more space than we had in Building T-4 and the Life Sciences Building, but we were crowded almost from the day we went in. We economized by not filling in the area over the pipes, vents, and the other things in the ceilings of the rooms, hallways, and so on, which everybody still comments on. We didn't have the animal facilities or the laboratories that we wanted and needed.

We kept trying to get additional money for funding but never obtained any from the federal government or private sources. Harvard, Hopkins, and almost every other school of public health had taken maximum advantage of the federal resources. The school in Hawaii built its whole facility based on half of it being federal money.

Finally, a proposition was put on the California state ballot for additional funding to the university, specifically for medical and health facilities on all the campuses. This was around 1967 or 1968, when I had first become the dean. We talked to any groups we could find regarding the urgency of this measure. Quite an organization was put together by the university. We did mailings. We talked to Rotary Clubs, medical societies, PTAs-- anyone we could get to listen--to try and influence the vote on the proposition. At that time, we knew if we could get the state money, we would have matching federal funds to build ourselves a large addition to Earl Warren Hall.

To make a long story short, the voters turned down the bond issue, and that was quite a blow. The next year, another issue was put on the ballot, renewing the appeal for money for this

purpose, and an even a larger effort was made. The politicking was all being run out of the medical center in San Francisco. Dr. Philip Lee was chancellor there at that time, and he was made the head of this community action program. President Charles J. Hitch of the university gave it considerable attention and support.

Meanwhile, I realized that there was not going to be any point to our school having money from the bond issue source if we didn't have a campus priority for a building and some ability to go ahead. So I did a lot of politicking on the campus to sell the importance of having our school be at or near the top of the priority list for a building if the bond issue passed. I was quite successful in this effort, and we went from something like tenth place on the campus construction priority list up to first or second place. I found support from many departments, including molecular biology, the virology laboratory, the bacteriology department, and so on.

Now, the only other unit on the campus that was going to benefit from this new funding was the School of Optometry. They had already been approved for a new building, had been given the planning money, had plans, and were ready to go. I spent a lot of time that summer on the road, talking to medical societies, Rotary Clubs, and any group that I could get to stand still that represented voters in California. Most of the audiences, when I would go to talk to them about our research, the school, and this type of thing, were more interested in what was going on on the campus with regards to the Free Speech Movement and People's Park, and they yelled at me that we were not handling the situation the way they would do it if they had their way. I was trying not to disagree with them too openly, because I needed votes. Many were rednecks in their approach who were sure they knew exactly how to handle the situation: "Bash the radicals." I didn't pretend I knew the way, but I knew I couldn't do it the way they wanted me to, which was to hit people over the head with a club. I usually was able to steer the discussion back to the importance of the bond issue for health and medical science facilities.

Anyway, the bond issue passed the second time around. I was very elated about this, because it meant that we should be able to get planning money immediately and would have a good chance of getting a new building with federal funding in the near future.

To my shock and dismay, after the bond issue had passed, a lot of money was given to the medical centers and the School of Optometry at Berkeley was funded for completion of their new building. Then there was a regents' meeting concerning construction, and they made a decision to save the taxpayers of



California some money and not use any additional bond money on the Berkeley campus for construction of facilities. I was crushed. This was in spite of the voters approving such building. My dream, my dedication, and my time and effort to raise the money had been successful in the sense that the money was approved by the taxpayers to build such buildings, including our school.

I wrote a letter complaining very bitterly to University President Charles Hitch about this treatment by his office and the regents. The response from President Hitch was, "I have resigned, and you'll have to talk to my successor about your problems." I wasn't too satisfied with that answer. But the next president [David S. Saxon], didn't feel responsible for past actions of the office, so nothing was done.

So optometry got its new building, and some people in the School of Public Health say, "I still hate the School of Optometry because they got their building and we didn't get ours." But I don't, really, because I'm pleased that they got something out of it, even though we didn't. It wasn't very long after that when the federal government again stopped matching funding, so we wouldn't have gotten it anyway, but we would have had a nice addition to our building that would have taken care of the needs of our student body, faculty, obligations in research, and so on, which we've never gotten. I'm quite confident at this stage that I'm not going to live long enough to see a new building or annex for our school. I don't know of any angel who is going to come along and drop ten million dollars or more in our laps. As a rule, public health alumni are not that wealthy.

So that's the story on bond issues. We made two attempts, finally one was passed, and it didn't pay off as far as the School of Public Health was concerned. When you're the dean of a school, you have no choice except to do the very best you can to try to get those types of fundings. If you fail you are disappointed, and your successors inherit the problem.

### Faculty Recruitment

Hughes: Do you want to talk more about recruitment of faculty?

Reeves: Yes, and I think I'll talk about it in a historical way. I think I talked earlier about when President Sproul was selecting the first dean of our school and tried to get Dr. Hammon to take that position. Search committees for a dean had not been heard of at that time. Whoever was in charge of the university or the campus



or of an existing unit just took it upon himself to find somebody who they thought was an appropriate person by reputation and by name and got him or her to come to take the position.

When the school started, we didn't have a large faculty, just a very small nucleus of a faculty. The president of the university was making the decisions about who was going to be selected to be a dean. When the first deans were appointed, their primary job was not to appoint a faculty or campus committee to help find a candidate; it was the dean's responsibility to search the country for the best person. One of the attractions of a deanship originally was that it could be a career position. For example, Dean Smith was a dean for over twenty years. Today, reviews for reappointment are done every five years.

Hughes: How did they actually do that originally?

Reeves: Certainly the dean would ask his faculty for suggestions, but he also used what was almost a clique in public health. The American Public Health Association was not that large a unit. There were only a few schools of public health. There was Harvard, Hopkins, North Carolina, Columbia, Tulane, and Michigan, and those were the schools of public health until ours was formed. There were no other schools in any other area and none in the West; there were no schools west of the Mississippi River. So it wasn't very hard to find out who the people were in public health, or which people were teaching in schools of public health or medical schools. It was a small community. Everyone knew each other.

The annual meetings of the American Public Health Association didn't have thousands of people attending its meetings. Today, at the end of a week of APHA meetings you haven't seen your best friends to say hello. In those days you saw everyone, and they didn't have the meetings split up into many sections on health education, medical care, environmental health, epidemiology, et cetera, et cetera. Everyone went to meetings to see each other, usually sat in one big room, and frequently had lunch with friends to get information.

So actually, by word of mouth, by reading the public health and medical publications and so on, a dean would be able very quickly through the grapevine to find people whom he thought were appropriate candidates to be dean for his faculty. That's how Dr. Smith or Dr. Rogers before him found people like Dr. Jessie Bierman, who was head of maternal and child health at the World Health Organization at that time; she was obviously a leading person in maternal and child health. As I told you, Dr. Dorothy Nyswander was found as a leading person in the field of health education, and Dr. Jacob Yerushalmy was brought in from New York

state as a leading person in biostatistics. So there was no difficulty in finding key people who could be the nucleus for starting a program in a particular field of public health as far as teaching and research were concerned. Once recruited, those people, in conference with the dean, would decide whom they wanted to add to the faculty in their particular area.

So it was possible in those days, if you were from a state-funded university that had assured money for salaries, to do a good job of recruitment. The school was new, so the administration of the campus was very anxious to have the best and frequently the most senior people it could get in order to develop the best possible school.

Now, this was the scheme, I would say, up to the time when controversies arose on the campus around the loyalty oath, freedom of speech, academic privileges, and those sorts of things. The faculty became more and more concerned about being represented in administrative decision making. By the time I became the dean, the campus protocol for recruiting new faculty had changed, and the dean was not the major person making decisions. That doesn't mean the dean didn't have any input, but you advertised that a position was available and had a search committee that had to be appointed from faculty within the school and from other campus academic units. However, our school was not just the Berkeley campus but was considered to be a statewide school, so we also had to have representation from the San Francisco campus as well.

You wound up with a small selection committee, which usually the dean appointed with approval by the chancellor. Even when I was dean, they had not gotten to the point where you did a big national and even international advertising with increasing concerns with equality, ethnic group representation and recruitment of a larger proportion of women on the faculty. On the campus there were many departments that did not have a single woman on their faculty. But even before I was dean, as I said earlier, a very high proportion of our faculty were women. We had Drs. Dorothy Nyswander and Beryl Roberts in health education and one male professor in that same group--Dr. William Griffiths. Dr. Bierman and Dr. Helen Wallace had been and were in charge of maternal and child health. Public health nursing and nutrition were always headed by women. So our school had a very high proportion of faculty who were women as compared with other units on the campus.

Oftentimes we attempted but could not find candidates for recruitment who were black or Chicano, because there just were not that many people in those ethnic groups who were being trained in



public health. That's still the case today. It's very difficult to find such people who are qualified.

But there had been a change from almost an autonomous selection by a dean of who the faculty would be, and frankly I still cannot see that the new methods that are in use have drastically improved the quality of people who are recruited. Basically, many choices almost become a compromise. Faculty are not being selected solely on the basis of their competence as academic or research people and so on, but frequently they meet some other requirement of our society and its special interest groups. It's an interesting change in process. It introduces the problem that sometimes committees can't agree within themselves on a choice, and yet the administration does not have the ability to intercede and make the decision. So it's very difficult.

Hughes: Please tell me some of the considerations in recruiting faculty.<sup>1</sup>

I will use epidemiology as a model, as I was closest to it. As we got into the sixties and up to the present time, it was been extremely important that we not have a faculty that represented a very narrow concern and interest on a particular and narrow set of diseases--say respiratory or heart disease. This would dictate to the student body what they were going to be exposed to. This is something that I've been very pleased that I was able to do. I was able to recruit Dr. R. A. Stallones, better known as Stoney, to the faculty in epidemiology. He was interested in both infectious and noninfectious diseases.

Stoney was a physician, had been in the U.S. Army, and was sent to us by them for an M.P.H. degree in 1952. At that time he had done a unique study on the epidemiology of heat stroke, but he was interested primarily in infectious diseases. I was able to recruit him in 1962. He then became extremely active in the studies of heart disease and developed a project in a senior citizen community down in southern California at Seal Beach to study heart disease in a senior citizen population. He also developed the Japanese-American studies, which focused interest on the differences in chronic disease, primarily heart disease occurrence, in Japanese in Japan and Japanese who had immigrated and gone through one or more generations on the West Coast in the United States or in Hawaii. So he rapidly developed a broadened curriculum which included chronic diseases, but he also participated in infectious disease research and teaching.

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<sup>1</sup>This discussion was moved forward from its original position in the transcript of this interview.



Then in 1968, I was extremely fortunate in being able to recruit two people to the faculty: Dr. S. Leonard Syme, who was interested almost exclusively in heart disease. He had a Ph.D. in social sciences. He was in the United States Public Health Service, where he was involved in the heart disease program of the National Institutes of Health. He has become a really outstanding person in epidemiology and brought the social science approaches to disease studies into the curriculum. This has become extremely important, particularly in heart disease and related diseases.

I was also able to recruit Dr. Warren Winkelstein, Jr., who was working at that time in the New York Health Department. He's a physician. Dr. Stallones had gone to Houston to be the dean of a new school of public health there, so I really needed a physician on our faculty in epidemiology. Warren had changed his interest from poliomyelitis and infectious diseases generally and had become interested in air pollution and the relationships of air pollution to cancer and other diseases in Buffalo, New York. He had become well known for that. He brought a whole new area of noninfectious diseases into the program but also still had a background in infectious diseases at a national and international level. These two people have risen to be leaders in the field of epidemiology. Winkelstein followed me as the dean of the school when I resigned. He is now back into infectious disease research with a large research program on AIDS.

People say, "Why didn't you recruit somebody else to work with you on bugs and viruses?" I say, "We had that, and we didn't need to supplement that area." What we needed was to attract good students and to develop a really good teaching program with a breadth of coverage. We recruited a whole series of people after that. Dr. Judith Cohen came in as assistant professor. She was trained in the social sciences and concerned with mental health. Dr. Carl Keller was recruited as assistant professor. He was a veterinarian interested in genetic approaches to diseases. The two of them no longer are here. And then we recruited Dr. Mary Claire King, again a person concerned with genetics and primarily with chronic diseases such as cancers. She's still here as a full professor. When I retired, Dr. Arthur Reingold was recruited to fill my position. He is a physician-epidemiologist. He had done very important studies on toxic shock syndrome and legionnaires' disease. He continues to have a broad interest in infectious disease with considerable activities in underdeveloped countries.

Dr. Steve Selvin holds a joint faculty appointment in biostatistics and epidemiology, which is a recognition of the importance of statistical approaches in advanced epidemiological studies. Currently we have Dr. William Satariano, whose primary focus of interest is on diseases in the aging population. Our

aging populations pose many unique health problems today. A search is currently underway for another faculty member in epidemiology.

So we have had a continuing evolution of the type and number of people in the program, and they represent a number of fields. But what holds them together is that they are all concerned with epidemiology--how to look at disease distributions in populations to determine why certain people are selected from a population to get a disease and other people escape it, and hoping that sort of basic knowledge will allow us to develop intervention methods to prevent disease. This unusually competent faculty with their broad interests is attracting a large number of very competent graduate students who have a diverse range of interests.

Hughes: Do you want to comment about the selection of administrators?

Reeves: The question of how this is done has bothered me since I was a student, and I commented on that earlier. I always wondered what determined who was selected to be put into responsible administrative positions on the campus. It seemed to me when I was a student that very frequently the best teachers and research people were selected to be administrators and did not continue to teach or to do research. As a student, I was very conscious of this. When Dr. Freeborn became associate dean for the College of Natural Resources, we lost the best teacher we had on insect morphology and a person concerned with research on mosquitoes. Sanford Elberg was taken from teaching and research in medical microbiology and put into administration almost full time. You could go on and on with examples.

I can take myself as an example. I'm not sure the fact that a person who has been a teacher and a research person makes the best administrator in the world, because we haven't had any training in administration. Most of our administration, frankly, is by the seat of our pants. I can't say this is necessarily a bad a way to do a lot of administration. However, there must also be ways of doing administration that we do not know.

## Teaching

### Teacher Training

Hughes: Isn't the same true of teaching?



Reeves: Teaching is exactly the same thing. It's a very interesting thing that practically no one on the faculty has ever been trained in teaching methodology or theory. By training in teaching, I mean a degree in education, a training program in the theoretical and most modern methods of education.

As a matter of fact, it becomes very humorous sometimes. A few years ago, the California Mosquito and Vector Control Association was having an educational program for their field employees. I was asked to spend a half a day with the mosquito control operators who would come in from the field--most of these people had a high school level education--and I was to tell them all about mosquitoes, viruses, and diseases. This course was set up down at Visalia in the local community college.

This was to be a short-term, two-day training course, and the organizers asked me if I also would be willing to be designated the faculty person responsible for the program that was going to be given in this community college. I said, "Sure, fine." At that time, I had been a dean, I'd gotten the distinguished teaching award from Berkeley, and I'd been teaching for forty years on the university campus. The administrator for the community colleges said, "I have to see Professor Reeves' teaching certificate." I didn't have a teaching certificate. He said, "Well, then, he can't be responsible for a course in this community college." I thought this was very humorous and still do.

It turned out that Dr. Robert Washino from the faculty at Davis, who is now an associate dean up there, had a teaching certificate and was also going to participate in the course. They had him be responsible for the course. He and I still laugh about the fact that I wasn't qualified but he was, and he's always rubbed that into me because he was a former student of mine.

The administrator for this community college came and listened to my presentation in the course. He sat next to me at lunch and was complimenting me on my talk. I said, "I'm glad you liked it because, as you know, I'm not qualified to teach here." He laughed, as he had forgotten completely about this, but he was just following the rule books.

#### Dr. Reeves' Academic Positions

Hughes: Dr. Reeves, would you say something about your progress from lecturer to full professor?



Reeves: Originally I was a lecturer at the medical school in San Francisco, which was just a second title that I had in addition to my research title. I had gone into the research series at the Hooper Foundation and been promoted from assistant research this and that to associate research this and that. The research series that I was in was parallel to the academic titles regarding salary except it was to do full-time research. So research assistant would be the same level as assistant professor; research associate would be the same as associate professor.

So I was a research associate at the medical center and had a lecturer appointment to teach tropical medicine. When they started the School of Public Health in Berkeley in the mid-1940s, I was given a lecturer appointment because Dr. Hammon wanted me to assist him in teaching the epidemiology courses on this campus.

The decision was made by Dr. Rogers when he came in as dean that I undoubtedly was going to become a full-time faculty member in the school, in a state money position. But he thought it would be proper if I had a degree in public health, because after all, at that time all I had was a bachelor's degree in entomology and a Ph.D. in medical entomology and parasitology. I had no formal qualifications or degree in public health. Dr. Rogers had one, Dr. Hammon had one, and a lot of the other faculty did. I couldn't object strenuously to that, because I even thought I might learn something about public health. I'd had a lot of experience in our research working with people in health departments but not in a formal fashion.

Well, I couldn't be made a regular academic person in the professor series as long as I was taking courses for credit. So I continued to be a lecturer until I finished my degree in 1949. I think I mentioned that I was teaching courses in epidemiology with Dr. Hammon at the same time that I was taking the same courses for credit, which created a few complexities but not insurmountable ones. I finished my master of public health degree in 1949. It was rumored that the reason it took me so long was because I wasn't too bright. However, the fact was that during those years I did a lot of traveling, chasing epidemics of encephalitis all over the U.S.A. and the world.

When I completed the M.P.H. degree, they decided to give me a regular academic appointment, so I never was an assistant professor. They just transferred me across from a research associate into an associate professor of epidemiology with tenure, which was nice. In addition, I was given an eleven-month rather than a nine-month appointment, which recognized my need to do research activity in the summertime. It not only increased my

salary, it took me off of research grant and contract funds, so we didn't have to raise that money for me to do research. An assistant professor doesn't have tenure, and when I was made an associate professor I automatically had tenure. Of course, I'd already been working for the university from the early 1940s up until 1949, so I had almost ten years in the university in the other types of appointments.

It was only a few years later [1956], when I was forty years old, that I was promoted to full professor, which is a very satisfying thing to have happen. So it was a really rapid evolution from a research appointment, to an associate professor with tenure, and then to a full professorship in the school.

### Teaching Epidemiology

Hughes: Was your teaching itself exclusively epidemiology?

Reeves: Yes, all I taught was epidemiology. There was a pretty strict separation of the teaching faculty into various subject areas, primarily because in the 1940s and early 1950s each of these areas had really developed into a science or recognition as a major public health area. Each area represented a large topic to be handled in the curriculum.

When the school started in the forties and the early fifties, and I talked about this earlier, we were in large part a retreading operation, because most of the students were health officers, sanitarians, and health educators who had been out working in health agencies but had not had formal training in public health. So we had a number of older students who came in until we got through what amounted to a backlog of such people.

We didn't have large classes, because our facilities were limited; we didn't have specialized degrees in epidemiology or in public health administration. There was only an M.P.H. in public health, and almost all the students took a pretty routine type of curriculum. They had to take epidemiology, biostatistics, environmental health sciences, and public health administration. So they had to cover at least four areas. As electives, they could have additional courses in health education, maternal and child health, nutrition, or whatever it might be. Each student had a curriculum of four basic core courses, and there was a sequence in some of those courses.

You were allowed to specialize to some extent by taking more than one course in an area. In my particular instance, I took all the courses available in epidemiology, which was two courses plus a seminar. And the same thing could be true in almost any area. The number of courses was limited because we had a very small faculty, and we had a relatively small student body. The student body in the 1950s was at the most forty or fifty students.

The basic lecture and laboratory class in epidemiology would be taken by all the people in the master of public health program. Most of the people were mature and very well-experienced, so we had small classes, and we didn't have a lot of support staff such as teaching assistants. Everybody who took the epidemiology course had the lectures, and they had a laboratory, which meant that they had to take data from outbreaks of diseases, analyze them, and then write reports as they would have if they were actually out doing a field investigation of an epidemic. This was a rigid and demanding type of experience. There was a lot of variation between individual students in their ability to handle these things, but everybody worked on these exercises.

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Reeves: Throughout the school, classes were small and intimate. You got to know every student in class very well. The professor was in the classroom, working with them in the laboratory. If you were fortunate, you would be able to get a teaching associate. I was very fortunate to get Flora Hanks, who had graduated from the school in the Department of Hygiene days and had done quite a bit of work in public health departments. She was a very good and steady individual who helped me to develop laboratory exercises and who could go into the laboratory and work with students to help them with elementary biostatistics, microbiology, and so on. So she and I would work together as a team.

In the late 1940s I was able to add a young person, Susan Anderson, who was taking the curriculum in public health but also was able to be a teaching associate. We even co-authored a paper or two together. Later, Fern French became the principal person we utilized in the laboratory. She was very well trained, had a doctor of public health degree in biostatistics, extensive field experience working on an MCH project with Dr. Bierman in Hawaii, and she was a very good laboratory support person.



### Introducing Courses on Chronic Diseases

Reeves: Originally there wasn't a lot of variation in courses. The epidemiology courses were almost all on infectious diseases. Chronic diseases had not been recognized as being as important as they are today. In a basic course in epidemiology we taught principles for investigation of epidemics, what databanks you have to have, what controls are required, and so on. We didn't spend a lot of time on those diseases that we didn't personally know too much about. So heart disease and cancer just didn't appear in our curriculum in the 1940s and 1950s. Dr. Hammon and I divided up the various types of communicable diseases, and our curriculum really was a communicable disease epidemiology type of program. Dr. Dwight Bissell, health officer of San Jose, taught a course in communicable disease control for undergraduates and people without an M.D. degree.

This began to change when Dr. Smith came into the program in the fifties. He had an interest not only in infectious diseases but also in diseases like diabetes, pellagra, and some of the other chronic diseases. So we began to broaden the program and to bring chronic disease types of problems into the program. But again, we were teaching basic principles of epidemiology and how you think your way through a problem--how you have to go out and get information to find out what population is at risk more than other populations, what factors control this, or whether it's exposure due to an occupational factor. However, we also began to talk some about genetic factors that influenced susceptibility to infectious or chronic diseases. But we didn't worry about covering all diseases.

There is a major change today, because each student seems to come to the school with an idea that there's a particular disease or group of diseases that they will be interested in for the rest of their life. In today's world, the majority of students' interests is not in infectious diseases. The students in the United States say, "These problems have been solved; infectious diseases aren't a problem anymore." That's not right, but they say so. They're interested in chronic diseases--cancer, heart disease, occupational exposures to health hazards, and so on. So student interests have changed, and the faculty has evolved into a very complex faculty representing different areas of science and disease expertise, with much of the emphasis being on noninfectious diseases.

### Changes in Course Format

Reeves: The other thing that has changed is the format of courses, because now, instead of having a student body of forty students, we'll have a student body of three to four hundred students in the school. It is still accepted by the accreditation groups that deal with schools of public health that everyone who takes a master of public health degree has to have a course in epidemiology. That means that we have a course which is taught for anyone who has to have an introduction to epidemiology. It's strictly a lecture course, and it may have almost two hundred students enrolled at one time. Now, that's a lot different than an intimate course of maybe twenty students sitting in a laboratory dealing with actual basic data in their hands, having to analyze it and determine why the disease has this distribution, and then write a summary report. The groups of two hundred students may or may not come to class each day. They sit there and listen to lectures. The lectures get duplicated by whatever the organization on the campus happens to be that sells such notes. These notes are taken by somebody they pay to take them. People can take a course almost as a correspondence course if they buy these lecture notes and study them enough to pass the exams.

However, we still have courses that are for the epidemiology majors or the students in doctoral programs who want to get enough epidemiology in depth to be able to be examined orally in that area as their first or a second area of qualification for the doctoral degree. Those current courses mostly are split up into infectious and noninfectious disease courses. There's a course given by each faculty person in the area or diseases of particular interest to that faculty person. As a matter of fact, it's almost a requirement that each faculty member should have the privilege of developing a course that's in his or her particular area of research interest. So Dr. Mary Claire King may have a course in genetics as it's applied to epidemiology, and Dr. Syme will have a course in applications of social sciences to epidemiology. Dr. Winkelstein currently has been giving a course devoted entirely to the epidemiology of AIDS, because it's such an important disease today. Dr. Arthur Reingold has a course in disease surveillance, Dr. Alan Smith a course in occupational epidemiology, and Dr. William Satariano has one on aging. I used to have one on arboviruses and zoonoses.

So we have a series of core courses that are given for majors, which can include a laboratory but nothing like we used to do. Everything now is on computers and being done by computer analysis of a databank. Students learn to pull the data out of the computer. There's a lot of difference between that and having



individual case histories in your little grimy hands to read and interpret.

### Strategies for Teaching Epidemiology

Reeves: Let me give you an example. There was an outbreak of typhoid fever in Olean, New York, years ago that became a classic. We had the original case histories of all the the hundred or more cases that were involved, which gave their age, sex, use of water, food, contact with other people who had the disease, and place of residence--all of these identifying characteristics. The students had to take that series of case histories, analyze them, and come up with an answer to the source of the outbreak. Their assignment was to write an epidemiological report. They did it, and it came out very interesting.

We were the only place a person could get training in epidemiology in the western United States. We had to develop our own databanks; we had to find epidemics that would serve our purpose. As a matter of fact, I still use some of the early epidemics when they ask me to give a lecture in a current course on malaria, typhoid fever, or encephalitis. I take the class step by step through investigation of the epidemic.

I'll give you an example. In 1952 we had a malaria epidemic in Campfire Girls up in the Sierra Nevada mountains in Nevada County. They wound up with thirty-some cases of malaria, and it was all traced back to a Korean War veteran who went up on the Fourth of July weekend and slept out of doors, where his malaria relapsed. The *Anopheles* mosquitoes up there bit him and then several weeks later went over and fed on the Campfire Girls and other people in the area, which resulted in all these infections. I'm making it a very simple epidemic or exercise.

Hughes: Yes, I'm sure it wasn't.

Reeves: It taught us an awful lot about the epidemiology of malaria. When malaria got introduced into a bunch of tender, receptive young ladies with a lot of skin exposed, we learned exactly how the parasite was transmitted and the characteristics of that infection. We learned it could have been controlled. We understood the importance of this being a reportable disease.

We call this a slide exercise because I put all the data onto slides. The epidemic started off with just a few cases of malaria that were reported to the State Health Department. Then you ask



the students, "With this information, what are you going to do? What information do you have to get?" Finally it comes to tracing down the original source case, which was not very easy. You spend a couple of hours with the students, looking at the data as it evolves based largely on the questions they asked. I have another exercise on typhoid fever in Kern County in the forties, an epidemic that occurred while I was down there working on encephalitis.

Well, by putting these data on slides and having them ask for specific information, you can make the students think through the problem and ask the right questions to get the next set of data for their analyses. So you take them into a situation that they would have to think through in the field: what laboratory tests they would use, how they would interpret them, and so on and so forth.

Hughes: It sounds as though your courses were very much geared to your own experience and what you do out in the field.

Reeves: It is almost like solving a crime. You don't just go in and tell a person, "This is what you do," and have them memorize a cookbook, because the cookbook frequently won't fit the case, and you want them to have to think it through. A primary approach that I had to teaching was to make students think if I possibly could. I think it's more enjoyable for them to have to think through and have a dialogue about a problem than to memorize facts, dates, and names of people.

Hughes: Was your teaching very interactive?

Reeves: Yes, very interactive. Frequently, a student might be wrong. I wouldn't hesitate to let them go way out on the end of a limb before I would cut it off. Sometimes I would be wrong. I'd make some snap judgment answer to some question and then have to admit, "I don't know," or, "I was wrong." But I never hesitated to say, "I don't know." I never tried to bluff my way through.

Hughes: How similar was your course year after year?

Reeves: A lot of the stuff was used repetitively, because it's very hard work to get together a good data set on such things. But at the same time, I tried to change a significant part of the course each year and to bring in current events on the disease being studied. Number one, it was boring to do the same thing over and over again; number two, it didn't stay in line with the times--the changes that had taken place.

Frequently I might use a laboratory exercise that was based on old data but then try to bring into that same classroom data on what was happening currently with that same disease. People say, "Well, typhoid fever doesn't occur anymore, so why should we learn about typhoid fever?" Then I would present to them a series of typhoid fever cases that have happened currently. It may be that people went into Mexico on a vacation and got it, but it was still typhoid fever, and it was still being brought back into California.

Our seminar for years has evolved in part around historical readings in epidemiology. We pick out publications that are landmarks in epidemiology because for the first time they present enough detailed knowledge of the disease to suggest a method to control it.

Examples of this would be John Snow's book on cholera, which he wrote based on investigations he did in London back in the mid-1800s. This was long before microbiology had evolved as a field. The cholera organism was not known, but Snow had thousands of people dying from cholera in London. He was able to work out that this was an organism that came from the human intestine. It got into and contaminated water, and that was a source of infection. He demonstrated that the sewage of London was going into the Thames River. Some of the water supply for London was coming out of the Thames River below the sewage outlet, so you had a built-in system that was circulating the cholera organism. It was a very efficient system. There were some water outlets that were above the sewage outlet and some below it, and he was able to differentiate those as sources or nonsources of cases.

Actually, Snow's works were classic studies of cholera which led to nearly all the methods of control we use today: boil water if you need to, have a clean water supply, cut the contact between cases and susceptibles, and so on. John Snow was able to write practically the same thing that is in the American Public Health Association's current handbook on control of cholera. Robert Koch's description of the cholera organism was done thirty years after Snow's classical studies.

Hughes: Amazing.

Reeves: A student will say, "So who cares?" Well, Snow developed methods of testing water to see if it was contaminated with sewage. He mapped the distribution of cases and showed the geographical location of each of them and their relationship to contaminated water supplies. He developed rates, using a numerator and denominator; that is, cases were the numerator over total population at risk. He developed many of the basic methodologies



the students take for granted now and that they may use in modern studies. They forget or never knew where the methods came from, and they forget to use them sometimes. So this seminar brings to their attention, "This is where it came from, this is the methodology. Don't think you invented it, because reinventing the wheel is easy to do but no longer is a great discovery."

In addition, we bring into the classroom what's happening with cholera today. You might say, "Nothing's happening with cholera today," but there's a huge epidemic of cholera going on in Peru, Colombia, Venezuela, and other areas of Latin America, with thousands of cases and very large numbers of people dying. Cases are occurring in Mexico; American tourists are getting cholera and becoming ill on their return home. There hadn't been cholera in Latin America for years; it got reintroduced. Coinciding with Desert Storm, the current war, was the report that cholera had appeared again in Iran, Jordan, and one of the other Arabian countries. So that was something for the armed forces to worry about, because the vaccines for cholera aren't worth much and you have to depend upon good sanitation. You bring those current events into the classroom for discussion with reference to John Snow's studies over a hundred years ago and what you can do today to control the problem.

There are a whole variety of approaches we used in teaching: slide exercises, seminars where the students have to learn about the history of the field, and students have to write a critical review of the literature on some epidemiological problem that they are interested in and might want to work on in the future. By doing a critical reading of the literature they learn of the methodologies, what the unanswered questions are, and whether the studies that have been done are valid or not. During the year, students have to complete the written paper. Sometimes it is the first paper the student has ever written. They then have to defend their paper before the class for an hour. Everybody in the class has a copy of the paper. The student doesn't present the paper; the other students and faculty have read it, and they ask questions and tear the paper to pieces.

Hughes: Good practice.

Reeves: So a little blood gets on the floor on occasion, and you may have to sprinkle a bucket of sand around to sort of mop things up; but it leads to a lot of dialogue, discussion, and broader education, because everybody picks a topic of interest to themselves. Then everybody else had to be interested in that topic, too--enough to read the paper and discuss it.

Hughes: Was anybody else in the school teaching in this way?



Reeves: Not that I know of. Today the tendency is more and more away from this method to the cut-and-dry stuff--the methodology and so on. I'm not competent to teach epidemiology in the modern sense to a class today. I can't do it, because it's become in large part a biostatistical approach to problems. The target may be to apply a significance test to determine if something's a little different from something else; and if it is statistically significant, should it be pursued further, and so on. I'm not a biostatistician. A lot of our advanced courses deal with study designs that will lead to statistically valid numbers and so on and so forth.

My approach has been--and I say this somewhat apologetically--that I've never really used a lot of statistical analyses in my studies. I tell students this, and they look at me and wonder what sort of a dodo bird I am, at which time I say, "My idea of a good significance test is: if the data hit me between the eyes, it's significant; and if it doesn't, we need more data." I know it's really not the way to approach problems, and I frequently ask Marilyn Milby, my biostatistician colleague, to run a significance test. But when she or somebody else does apply statistical tests to our data, the differences usually are significant, because your intuition is enough to make sure they are. When we deal with bugs, we don't deal with ten people or twenty people; we deal with a hundred thousand insects. We usually don't deal with small numbers.

I don't have the feeling that the current epidemiology curriculum focuses as much upon principles of epidemiology in identifiable fashion as it does on the methodologies. I may be overcritical in that regard, but it's an interesting evolution in teaching that's taking place.

I said each member of the faculty could develop a course around his or her own particular interest in epidemiology. I had a favorite course that I taught for years on zoonotic infections, many of which are transmitted by arthropods. Zoonotic infections are infections of animals transmissible to man, and the arthropods are bugs that carry viruses, bacteria, and other parasites. I taught a whole semester course based on just those diseases.

Now, this isn't a topic or the sort of course that you get a hundred people coming into. I mean, there might be a half a dozen people, or I might have a semester where only two people are interested. If it was two people to six people, I'd have them meet in my office; I wouldn't even have a classroom. If it was ten or twelve people, then you had to have a classroom. When it was two to six people, I would say, "Okay, you guys pick out what

you want to talk about next week, get prepared, and come in. I don't even want to know what disease you want to talk about, as long as it's a zoonotic infection or an arthropod-borne disease. You read about it, and I'll take you over the hurdles when you come in." Those classes were fun. You saw the picture in my study at home of the class of six students sitting out on the lawn talking about such a problem. You have a lot of interaction in those situations.

We utilize the people at the State Health Department as much as we can in epidemiology teaching, because those are the people who are right there in the front line seeing new diseases or old diseases that are emerging each day.

Hughes: So you'd have them give a lecture on their particular interest?

Reeves: We have been doing that regularly. When Dr. James Chin was the head of the communicable disease division [at the State Health Department], at a time when I was extremely busy doing some other things as dean, he said, "Let us develop a course at the State Health Department, we'll teach it over here, your students can take it for credit with us, and you come in and give us some of the stuff on the arboviruses." Which we did. There were enough changes over there when Dr. Chin went to the World Health Organization that we developed a joint course, to which I added the zoonoses and arthropod-borne diseases. About thirty or forty students take that course for credit. Dr. Art Reingold, who is on our faculty and interested in infectious diseases, now organizes that course each year. The State Health Department, Drs. Ronald Roberts, Ben Werner, and other people are giving about half the course, and people from our side are giving the other half of the course.

Dr. Reingold took my place as a faculty member in epidemiology. He's an excellent person in that he has a broad interest in infectious diseases. He did the original work on toxic shock syndrome when he was at the Centers for Disease Control. He worked on Legionnaire's Disease when that was a brand-new disease that had just emerged. He's interested in AIDS. He's been working on measles vaccine programs in underdeveloped countries in Southeast Asia. So he has very broad interests and is carrying on the torch of infectious disease epidemiology in a very nice fashion.

### Thoughts about Teaching

Hughes: Dr. Hardy was very complimentary when I spoke to him. He said the reason that the epidemiology program is number one in the world is because of you.<sup>1</sup>

Reeves: That's very kind of him to say that. He's entitled to his opinion, and I'm too immodest to argue with him. [laughs] Actually, that's a very nice compliment, but you probably shouldn't say such things to me, because then you will find it very hard to live with me. [laughter]

Seriously, my response to your statement is, "You shouldn't be teaching if you don't enjoy it." If you can't talk about the things that you're really interested in, and with a great deal of enthusiasm, you're in the wrong ball game. I think the people that I've named in epidemiology during these interviews are really enthusiastic about their science, they're enthusiastic about this as their career, and that should be infectious to students.

Now, I get very discouraged, I admit, when I run into students who don't seem to be very excited about it, and they're doing it just because they're getting a union card or to meet a requirement for a job. And there are such people. There are times, I guess, when I'm extremely difficult with those people. I don't take to them kindly, I have little respect for them, and I'm not a person who hides my feelings about such things very successfully. I don't try to. I think one of the reasons that I've been able to influence some people's careers is that I tell them the facts of life and how I feel about such things. Perhaps I do it too much. And I'm sure I've turned off some of them, too. I'd be surprised if I hadn't.

It's awfully hard to talk with you about something like this without it becoming pretty mundane, but an enthusiasm for your field is important. And then the other thing is that you do get opportunities to carry your feelings about these things outside of the Berkeley classrooms. For a while, as I described, we had this extended teaching program in which we were taking an epidemiology course out into the communities. So you had a chance in a night school sort of a situation to instill some of this same enthusiasm in people in health departments who were taking an M.P.H. with us.

I have had a lot of invitations over the years to go to other campuses to give a lecture on one of my favorite topics. I'd go

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<sup>1</sup>Interview with Dr. James L. Hardy, November 16, 1990.



to the medical school in San Francisco. Now, I swore when I left the medical school that I'd never go back to that miserable place and teach a class again, because I'd learned to despise the students.

Hughes: Why was that?

Reeves: They were so rude. Most of them were so completely uninterested in what they were taking, merely because it was a required course. You could stand on your head, and they wouldn't watch you. They would sit there and read newspapers. They would get up and leave in the middle of a class. They'd ask unnecessary questions to be difficult. I didn't like it. I guess that was why I was glad to come to Berkeley and get away from it. But in recent years, I've been invited back to the medical school and found that the students were enthusiastic, they didn't sit and read the newspaper, they were excited about the course, and they said they appreciated my coming. So it was fun to go back and talk to them.

I go up to Davis to give lectures at the veterinary school and in the entomology department any time they ask me to. I have not been invited to the medical school at Davis. I go down to UCLA and do the same thing. I get invited to the school of public health at Seattle to give a class on whatever I'm interested in at the moment. I go to the school of public health in Houston, Tulane, or out in Hawaii. So I have had plenty of opportunity to meet other classes.

#### Student Contact##

Hughes: Was it different teaching master's degree and doctoral students?

Reeves: Yes, very different. In the years that I've been here, I've had over six hundred students in the epidemiology curriculum, either taking a master of public health, doctorate of public health, master of science, or a Ph.D. in epidemiology. Most of those six hundred-plus students were taking the M.P.H degree and were here for one year or maybe two years. So you had a limited contact with the M.P.H. students.

In contrast to that, I probably have had contact with close to sixty doctoral students. By contact, I mean I've been on their committees or been their advisor. I've undoubtedly been the chairman of committees for forty students. You have an intimate contact for at least two or three years with those students when you're their chairman. We try to get our students out of here

within a three-year period if we can. We don't believe in five-, six-, eight-year Ph.D. programs. You have office sessions that are one to one, and the sessions can go on for long periods of time. Sometimes you are working out a plan of study, others you are giving them some of your experiences, and so on.

In the large classrooms, sometimes the faces just sort of blend together, so you can't keep individuals clearly identified unless they're causing you trouble or asking particularly good questions. You don't get to know them intimately. But when you get away from that large classroom into the intimate seminar or office type of relationship, then you really get to know people, and they get to know you. You don't forget those people.

### Student Research Topics

Reeves: The really gratifying thing is to have students who are going to do research in an area directly related to your interest. But it's not limited to that. For instance, I've had students who have done theses on topics that are a long way from mosquitoes and mosquito-borne viruses. Karla Damus did her Ph.D. thesis on sudden death syndrome in infants. That's hardly a problem that is close to my area of research. Barbara Thompson, in her Dr. P.H. thesis, reviewed the success of blood bank screening for hepatitis viruses and asked the question: has this been a worthwhile public health program? A student was sent here from Europe, Dr. Louis Molineaux, who did his Ph.D. study on meningococcal meningitis in tropical Africa and used data from there that he'd been able to get his hands on. He's now the head of a big program on malaria for WHO in Geneva. Robert Worth studied eczema among Chinese infants in San Francisco and Honolulu. I could go on and on. Whatever the thesis topic, it was well done, and the student was interesting.

So you're not limited to students doing studies in your particular area of research, but you still have an interest in their methodologies and in getting them through the epidemiological approaches--although they'll become much more sophisticated in this particular disease than you are.

While the majority of my students have worked on projects related to viruses, mosquitoes, and things of that type, they usually are working on the periphery of our major research project aims. In other words, when we have a research grant to work on a particular problem, we have a laboratory and a field staff working on that, so the student picks off research topics from the edges

of this project to do for a thesis and does not study the central theme.

Hughes: Does the student really pick, or do you suggest?

Reeves: With one or two exceptions, I have never suggested a research project to a student. My philosophy has been that if a student can't come up with something that is worth doing, it shouldn't be their thesis. I've been fairly rigid on this. The few exceptions have been student Ph.D. projects in which something suddenly happened where the project they were on couldn't be done.

An example of the exception was a student, James Olson, who was very interested in coccidioidomycosis (valley fever) and designed a beautiful study to be done on navy recruits in the Central Valley at a navy base in Tulare County. He had the whole study laid out, and it was going to take about two years to do it. He was a commissioned officer in the U.S. Navy, and the navy suddenly decided it had a vacancy in Southeast Asia and was going to move him out of here. So he was suddenly stuck. He obviously couldn't do his two-year field study; he only had eight months left to complete his degree. So I gave him a databank, and we discussed a problem that he could approach and analyze with this databank. He did a very nice job, which I hadn't had the time or staff to do. As a matter of fact, a great deal of his findings are referred to in the monograph we just finished. His thesis was a major reference in the monograph, and I referred to it in an earlier interview with you.

But I've been happiest when students came in and said, "Look, this is what I'd like to do." Dr. Richard Emmons, who is now the director of the state virus laboratory,<sup>1</sup> was in our Ph.D. program; he already had his M.D. degree. He came here after being at the London School of Tropical Medicine. When he'd finished his course work and passed his exam, I said, "What are you going to do for a thesis problem?" He said, "I want to work on the overwintering of Colorado tick fever."

I said, "Dick, I don't know much about Colorado tick fever, and it's not part of our research." He said, "I know that, but I'm interested in Colorado tick fever. That's what I want to work on." I said, "Okay. Where are you going to get the facilities? We can't put you in our lab to work on ticks and viruses." He said, "I'm going to do my research at the state health department." He had it all worked out. He'd found his support,

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<sup>1</sup>The Viral and Rickettsial Disease Laboratory, California Department of Health Services.



he had found the laboratory, he'd found the people who were interested in working with him. He did a very nice thesis on Colorado tick fever and still maintains that interest today.

### Scientific Writing

Hughes: Were you demanding in terms of the written thesis?

Reeves: I tried to be. I guess I have a reputation for being a bear on scientific writing. The reason I feel that way is that, for me, the hardest part of being a research worker is having to write up your data once you've completed a study. I find that is very, very difficult to do. It's fun to do the research, it's fun to have the idea, but writing is just hard, tough work. I really haven't a good background in writing. I have worked with a number of professional editors who tried to get me on the straight and narrow, sometimes successfully and sometimes unsuccessfully.

However, there's no point in doing research just for your own satisfaction. You have to spread the information to other people. That's what research should be about, not just to satisfy a personal ego. If you've done good research and you don't publish it, it's a wasted effort. I've had a few students who have never written a paper except their thesis, and I don't like that. Number one, it makes us look bad. Number two, there's nothing to show in the published literature that they ever really did anything worthwhile. Also, someone else may come along and repeat what they did without knowing it instead of extending the study. And I don't like that.

Hughes: But that's the exception?

Reeves: That's the exception. Most people want publications, because that's a critical part of their scientific career development. The other extreme of doing nothing is Roy Campbell, who just finished his thesis a year ago. I think by the time he left here, he had five papers which were accepted by refereed journals. As a matter of fact, one just came out in the American Journal of Tropical Medicine that came to my desk yesterday.

Hughes: What are you striving for as you write?

Reeves: Obviously, you want to present a picture of what you've accomplished in an organized and succinct fashion that is understandable to the reader. If it doesn't present the data and

methods clearly enough that somebody else can duplicate that research, you haven't achieved your objective.

Hughes: Had the students gotten some preparation in putting together a scientific paper before they came to you with their thesis?

Reeves: Most of them have not done so on a major study such as a thesis. The student frequently goes to the collection of theses we have by other students to look for a model, and sometimes I have to tell them, "I think you've picked the wrong thesis to use as your model." So you have them read a "best" thesis.

For a student to enter our Ph.D. program, he has to have the equivalent of our master's degree program, which means that in most cases he has written a term paper which was a critical review of the literature. In the past, they used to have to prepare research grant applications on a problem that they might want to do sometime in the future and present a plan for such a study. We don't do that anymore, because some students didn't want to do it. They said, "I'm not that interested in research." Well, what do you do? You can't argue with that. There is little to be gained by argument.

When teaching our laboratory in epidemiology, I edited every lab report for organization, grammar, presentation of figures and tables, and so on. I edited every laboratory exercise a student did; I didn't have a teaching assistant do it. I might have had a fellow faculty member do it, but not a teaching assistant. When I was reviewing student term papers that represented a critical review of the literature, I edited page by page by page. I stopped using a red pencil. People said that made people mad, so I used a blue or black pencil. The fact that they couldn't read my writing didn't make any difference, because they still knew I wrote a marginal note that wasn't happy about something, and that was enough. I do this sometimes with some trepidation, because I'm not sure the students' ego is going to take it and benefit from it. But the interesting thing is how many of these students come back to me now and say, "I have a copy of this paper or that paper or a page of my thesis which you edited in a frame over my desk." [laughs] And that pleases me when it happens.

I edit every page of every thesis I am on and usually several drafts. It's a lot of work to do this. Some faculty, I'm afraid, don't want to spend that much time on editing a thesis and making the student go through two or three drafts before it's acceptable. When finished, sections of a thesis ought to be acceptable as separate scientific papers, and indeed they usually are.

Hughes: What about commenting on the oral presentation of the thesis, and is that the way you had to do yours?

Reeves: No, I didn't have to make an oral defense. We've never had a requirement for an oral presentation as a basic part of our degree program for a Ph.D. They may still do it for a Dr.P.H. thesis. But the student has almost always by this time made presentations of his thesis in seminars. We try to have them make a presentation to a seminar group early in the thesis work and then, as their thesis is approaching completion, have a second seminar presentation to other students and faculty. By this time they're usually ready to give a presentation at a national meeting, and we try to encourage them to do so at the annual meeting of the American Society of Tropical Medicine, the American Public Health Association, or the Society for Epidemiological Research.

#### Further Thoughts on Teaching Epidemiology

Hughes: In 1981, you got the university teaching award. Do you remember anything specific about the citation?

Reeves: I really don't, except that it was an unexpected honor. One of the things I had to do when I was nominated for that award was to provide a statement of my goals in teaching epidemiology. Dr. Leonard Syme was the chairman of our department at that time, and this was one of the things he had to have as part of the documentation.

I find requirements like this are not very easy to respond to, because I don't really approach teaching or my research in that organized a fashion. I don't sit back and think, "What are my goals or philosophy in teaching epidemiology, and how do I put these on paper?" I felt that my academic training in medical entomology, parasitology, microbiology, and ecology, along with the early teaching experience I had at the medical school and in the entomology department as a lecturer and teaching assistant, gave me an unusually good basis for teaching the epidemiology of infectious diseases. That's what epidemiology does: it brings together the knowledge of a number of areas of science and focuses them on why this disease occurs in the pattern in which it does in populations.

Also, I had the opportunity to sit at the feet of some unusually competent teachers of epidemiology--Drs. Hammon, Smith, Meyer--and all were excellent teachers. They taught by using examples of their own basic experiences in studying diseases. In



other words, they didn't depend upon a textbook. I've been approached a number of times to write a textbook in epidemiology. I couldn't care less about writing a textbook. It would be boring to me to do this. I've never found a textbook in epidemiology that was so good that I wanted to make my students read it. I'd rather approach the subject in a different fashion.

Hughes: So the students worked from the lectures and the notes they took?

Reeves: That's right. And from the discussions, wherever they might go, and reading the classical studies in epidemiology.

People get this funny idea that the distribution of a disease in a population is a chance sort of a thing. It isn't just chance. It's not a random event with somebody up there rolling dice and saying, "You're next." There's something individuals do or where they are that leads to their becoming ill. There may be something in their makeup that makes them more susceptible than another person. So I like to deal with the fact that there's something controlling disease distribution.

On the malaria slide exercise, I like to say, "What is the unity of these cases?" Well, it turns out that the first four cases are all white, they're all female, and they all have their onset of disease within a few days of each other. It's the same disease, proven in the laboratory, and it turned out that they were all in the same place on the same date, two weeks before their disease onset. That's unity, and that begins to explain their being infected with malaria. They lived in Vallejo, Sacramento, and Alameda County, so where they lived probably had nothing to do with it. If you really look at the distribution of disease in a population, if you're fortunate you'll find a unity-- by age, by sex, by where people live, or whatever it might be-- showing that will explain how this disease outbreak happened. The acid test of your knowledge is whether the method you propose to control the disease works or doesn't work. If it doesn't work, you're wrong, and you just have to face that fact.

I don't like to take the position that some people take: "This is the answer." And then they say, "No, that isn't the answer. Now this is the answer." About the third time they go that circle, they realize they don't really know what the hell they're talking about. I won't reveal the name of the Nobel Prize winner who I used to bring here to lecture on a disease, and I won't mention the disease because it would identify him, and there's no purpose to it. But this person in his research, several times in a row, was dead wrong concerning the cause of the disease. First, it couldn't be an infectious disease, and it turned out it was. Second, it was genetic, and it couldn't be

genetic. When this person gave a lecture, he would not let a student put in a word edgewise. He would start talking, and it was simply, "Listen to me."

Hughes: So you didn't invite him back?

Reeves: Well, I invited him back because he was always an interesting lecturer, and the students enjoyed meeting a Nobel Prize winner. The last time he lectured, I set up the class before he came. I gave them a whole series of his reprints before he came and had the students read them, because this took them through this whole story about "This is the answer," "No, it isn't the answer; this is," and so on.

So here's this student during the lecture, waving his hand frantically in the back of the room. The lecturer didn't pay any attention. I said, "Hey, just a minute. A student has a question back here." The student said, "Doctor, I'm just a beginning student in this field, but how can you be so wrong so many times, and then be right, and not be criticized for it? Why doesn't that make you really not so good?" He said, "Don't worry about that. If you prove yourself wrong, it's all right. Don't let somebody else prove it; that is very bad." [laughter] He went right on lecturing.

And I guess the other thing that I tried to instill in students, because it had become so important to me, is the importance of reporting diseases to a central place. This can be very important, as you can't really understand what many diseases are doing, where they are, or study their epidemiology if it's not a reportable disease. Maybe you can if it's a very isolated incident, and you can get out there. Most people don't understand the importance of reporting diseases. Many people in our classes are physicians, and they have to understand that if they don't report the diseases they see that are reportable, nobody ever knows they occur.

Hughes: Do you think you got through to them?

Reeves: I think I did to most of them. As a matter of fact, some of that came home to roost later, when some former students stood up and told their colleagues how important it was when an epidemiological study was being done in their community.

One of the principal things I have learned is that no matter how deep a knowledge of the epidemiology of the disease you have, it doesn't assure you it's going to be controlled. You may know all about the disease, and you may know how to control it; but if society doesn't accept the importance of a control program, if the



people who handle the economics--namely the administrators and legislators--don't accept it, your knowledge isn't going to mean a lot.

Hughes: Yes. Look at AIDS.

Reeves: Well, we don't have the necessary knowledge of how to control AIDS except by what amounts to a pretty stringent dictum--stop having sex. We can stop blood transfusions being done until donor blood has been screened. We learned earlier with many other sexually transmitted diseases that you can't just tell people to stop having sex; it doesn't work. And you can't tell them to only have safe sex, because what they think is safe sex may not be safe sex. So such actions may dampen the epidemic, but they haven't really controlled it.

I had a whole series of items that I put together for Dr. Syme, who was documenting my nomination for a teaching award. I guess there was a philosophy buried in the document. but it comes down to having an understanding of the principles and methods of a field. You can present them to your students, and then you must sit back and wait to see if they do it in their careers. If they do and it works, you feel that you've been right. To be very candid about it, you must believe in the thing that you're doing, but you have no proof that it's right and going to be effective until your students successfully use the knowledge.

### Colleagues

Hughes: Do you want to say anything more about your associates as teachers?

Reeves: I stressed the fact earlier that I brought in Drs. Longshore, Syme, Winkelstein, Stallones, and others because they represented areas of and approaches to epidemiological studies that I felt were not being covered adequately in the curriculum. I brought them in because I'd had an opportunity to hear them make presentations; I knew they were effective speakers, I knew that they were stubborn about their beliefs in the field of epidemiology, and I felt they would be effective teachers. I also felt they could effectively interrelate with students either in the classroom or in guiding their research, and indeed, they have been successful. Dr. Syme still questions me regarding why I, an entomologist, recruited him to be the first social scientist on the faculty in epidemiology of the School of Public Health. My



only answer is that we needed him, and he was good. He still thinks I hired him because he wasn't a threat to me as an entomologist, and he is wrong!

It's a real pleasure to work in an environment in which you can get a team like this together. I call them a team, not in the sense that they're a team that somebody's steering as much as it is they're a team that is willing and does work together towards a common objective. I guess if there's any real strength, it is that we don't necessarily agree with each other, but we also don't have a feeling of intensive competition between us. I think that's probably important. We understand each other, and we respect each other, but we don't feel we're competing with each other for some resource or fame. We don't even feel we're competing with each other for students, and that can be a real risk. When the graduate division puts a limit on how many students you can have in your curriculum, and you don't have as many new students coming in as you have faculty members, then the faculty begins to get worried.

My philosophy also has been that students are not to be slaves for the faculty. I feel very strongly about that. Whenever I see it happening, I object to it strenuously and loudly. I think we have to teach our students independence in their thinking, because when they walk out of here with that degree, in most instances they're not expected to take a job where they're going to be somebody's slave. They're expected to be independent investigators. And if they've never done anything independently here, how do you expect them to do it when they get out there?

As a matter of fact, when students come to me about job opportunities, one of the first things I get into is, "What do they expect you to do on the job? Are you going to be somebody's servant, working under somebody to achieve their objectives, or are you going to be allowed to develop something of your own?" If it's just being somebody else's slave, I'll say, "What's your future?" As a faculty member, I think it's one of my obligations to bring this to the students' attention.

### The Importance of Publication

Hughes: How important are scientific publications to a career?

Reeves: Some people seem to have a complete mental block on getting their research published. It isn't a matter that they haven't done good

work; it's a matter of getting it written and published. When a person comes in as an assistant professor, and four years later they haven't published their thesis, you've got problems. Because if they can't get a paper or two out of their thesis, what have they done or not done?

Hughes: Would a person such as this have trouble with promotion?

Reeves: It isn't a matter of trouble with their promotion. They're dead. At the end of three years, an assistant professor is reviewed by a committee of senior peers. If they haven't made satisfactory progress in research or teaching, they are warned that by the end of another year their appointment will be terminated. That's pretty drastic.

Some people say, "Fine. I'd rather go somewhere else and do something where I don't have to publish." Other people want to fight it. But if your peers say you're not competent, even if the students may think you are the greatest teacher they ever had or ever will have, you have not attained the standards expected for promotion to tenure. One of the difficulties is to what degree student opinions are reliable in this regard. Students may like a person as a teacher because he's funny. They may like him because he appeals to them as an individual, or the person may indeed be an excellent teacher. These criteria are a part of evaluation but not the final criteria for success as a professor.

### Demands on the Faculty

Hughes: Do you see a problem with the multiple demands that are placed upon a faculty member nowadays, having to be a researcher, a teacher, and in some cases an administrator?

Reeves: Sally, I'm all for those expectations, and the reason is that it's a very hot oven you are in. You survive that hot oven, or you don't survive it. It may not be a fair demand on the person, but it certainly makes them achieve to the best of their ability. I can't oppose that system, because I think it's a tough world we live in, and those expectations are attainable.

You're expected to do outstanding, fundamental and imaginative research, you're expected to do significant community service and to be a good teacher. They say that each of these three activities is almost equally important, which is not really true. When it really gets down to the crunch in the academic world today, it's still research which is the major achievement.

Hughes: Research and publication?

Reeves: Well, that's the same thing. Research is not done until it's published.

Hughes: All right.

Reeves: But community service can eat you up--traveling to Washington, traveling to wherever you're going, being a consultant, being on campus committees. They can eat you up. Anybody who thinks you can do this in an eight-hour day for five days a week is absolutely insane, because you can't. Particularly in the younger years of your career, you may have to sacrifice a lot of things that you would rather be doing, such as spending time with your family, hobbies, art, etc., etc.





## IX MISCELLANEOUS

[Interview 12: July 19, 1991]##

### Memberships

#### The American Society of Tropical Medicine and Hygiene

Hughes: Dr. Reeves, today we're going to talk about societies. The first one is the American Society of Tropical Medicine and Hygiene.

Reeves: I joined two societies very early when I was a graduate student because they seemed to be most closely affiliated with my professional interests. They were the American Entomological Society and the American Society of Tropical Medicine and Hygiene. Of those two, it developed later that the American Society of Tropical Medicine was really the one society that I put most of my interest and activity into. I've been comparatively inactive in most of the other societies.

I joined the tropical medicine society back when I was a graduate student, probably in the very early 1940s. During the war I started going to their meetings when I could. I've gone to almost all of the meetings since the end of World War II. Originally I just went as an interested person, but I also was able to get papers on their annual meeting programs, so I had the experience of getting to know many of the people in the society by making presentations about our work. The society was the principal organization concerned with fieldwork on arboviruses, and this included the Rockefeller Foundation work I spoke of earlier.

Hughes: You were active in that particular organization because its members were interested in arboviruses?

Reeves: Their journal was a very logical place to publish on arbovirus research. It might seem peculiar that the Journal of Tropical Medicine and Hygiene would be a logical place to publish research we did in temperate or subtropical regions of California. We have done some work in the tropics but not much. But we're working on viruses that are so closely related to the viruses that are prevalent in the tropics that anything we do has relevance to people working in the tropics on their viruses, and what they're doing in the tropics has relevance to what we're doing. I would say the majority of the world's publications on arthropod-borne viruses are in that journal. The basic molecular biology studies usually are found in journals like Virology, because they're not related to the history of the disease or even the occurrence of the disease.

One day the ballots for election of officers for the Society of Tropical Medicine came. I know it was sometime in the 1950s. I was listed on the ballot to be put on the governing board of the society, and I didn't even know I was nominated.

Dr. Louis Hackett, retired from the Rockefeller Foundation, about whom I think I've made reference to before, was on our faculty at that time. He came to me and said, "Bill, I didn't know you were well enough known to even be put on the ballot." I said, "I didn't either, and I don't know how I was put on." He was on the council at that time because he was the editor of the journal. He came back later and said, "Hey, you're not only well enough known, you were elected to the council." That was a pleasant surprise.

Hughes: Do you know why you were nominated?

Reeves: I was active in the society, coming to the meetings and so on, and obviously I was interested in tropical medicine. By that time I had also probably gotten into Panama and other tropical places.

I was on the council for a period of two or three years--I don't remember how long the term was--which gave me a lot more intimate knowledge about the operations of the society, the way they handled the journal, et cetera, et cetera. I was also on the editorial board for the journal for some years.

The next thing I knew, in 1963 I was nominated as a candidate to be president of the society. Two of us were nominated for presidency of the society. One was Dr. Thomas Weller from Harvard University, who was a Nobel Prize winner with John Enders and Fred Robbins for their work on polio. He went into parasitology and tropical medicine after he'd finished the work on polio. We were good friends. When the ballot came out, I called him up and said,



"Tom, for God's sake don't vote for me. I'm voting for you." He said, "Don't vote for me." [laughs] It turned out that neither one of us was very excited about being president of the society, because it was a lot of work. So he was elected, and everybody commiserated with me that I wasn't elected, and I just grinned from ear to ear, because I really didn't want to be president.

And then in 1970 I was put on the ballot for the presidency again. I was elected the second time around.

Hughes: What kind of work did being president involve?

Reeves: You never know in these societies; it sort of comes down to whatever the president decides to make his cause. It wasn't a time of activism in the sense of societies becoming involved in politics as it might affect medical research or whatever the cause might be. All of us were too inexperienced in political action at the national level to influence budgets or whatever.

The previous president of the society, Dr. Telford Work, was particularly interested in international health. A report had come out from the United States Government Commission on International Development entitled, "Partners in Development." It was called the Pearson Report. I assume that was the name of the chairman of the commission. At any rate, the report did not give proper attention to tropical medicine, medical, or public health concerns, so a number of professional organizations took exception to it, including the American Medical Association, the American Public Health Association, and our group, the American Society of Tropical Medicine and Hygiene. These groups were organizing a "Task Force of International Health." Dr. Work had taken this as his particular interest when president of our society.

Anyway, with this background, when I became president one of our causes was to make sure the World Bank took proper actions to assure their projects were not having adverse health impacts.

### Working with the World Bank

Reeves: At the time I was president, [Robert S.] McNamara was the president of the World Bank, and they were getting a lot of money from the United States and other governments for development of resources in various countries of the world. A large number were hydroelectric developments or development of new water resources, not just for power but for irrigation and agricultural development. For practical purposes, they were funding these

developments without any reference to their possible effect on the occurrence of disease. So in developing an economic resource which was so important in Third World countries, they frequently were altering the environment so that it favored an increase in disease, whether it was malaria, schistosomiasis, ocular filariasis, or any other of these tropical diseases.

Members of the society urged the society's council to take this up as a cause. They felt the World Bank should be forced to employ some people who would understand the possible disease outcomes of these developments and that the bank should not be giving money for such developments unless this was done and an impact report developed. What they wanted was a counterpart of today's position of the Environmental Protection Agency (EPA), having many requirements to determine the environmental impact of developments, even though they may not necessarily be focused on disease. Our society decided we were concerned with the diseases that would result from these developments, and we thought it should become a major project to influence the World Bank to have such a concern.

As the president, I became the spearhead to initiate action. We only knew one way to get at this, and that was to go directly to the World Bank, which was not responsible directly to the U.S. Government. It's an international agency. It's sort of like the WHO in that they're free of any direct political direction by a single government. We didn't know any better way to approach the problem than to ask for an appointment with McNamara. What the heck, go to the top.

It turned out that Dr. Work and I had an extremely brief session with him. He recognized that he should be giving some attention to the problem or he was going to have some difficulties. He delegated top people on his staff to meet with us to be briefed on our concerns. I went, and other people in the society went. Telford Work, the past president, went with me on one visit, as did Dr. Don Heyneman from the medical center in San Francisco, who was a parasitologist and very active in the society. I remember the three of us went at one time to meet with these people, have lunch with them, and talk about these things. We were successful in getting the first person, Dr. James A. Lee, employed specifically to have a concern about disease outcomes. He was trained in ecology and had enough biological background to be able to understand what the disease problems were that we thought should be looked at. Ever since then they have at least paid serious lip service to the importance of diseases, but they continue to make many of the same mistakes.

This was the beginning of the World Bank being concerned with developing information and education on the impact of developmental projects in the Third World on disease problems. That really was the only major project we had during the year I was president. That took a lot of time in visits to Washington and a lot of correspondence. The records of this project, if they still exist, probably are in the Archives of the American Society of Tropical Medicine and Hygiene at Harvard University.

Hughes: Does the World Bank consult with authorities such as yourself?

Reeves: They never consulted with me. I'm sure they must at times have employed consultants.

Hughes: With expertise in your area?

Reeves: Arboviruses as such did not come up as a major consideration here but certainly other tropical diseases have. The Aswan Dam is a beautiful example of the problems. The Aswan Dam was built to regulate the flow of the Nile River and generate electricity. One unexpected outcome was an increase in schistosomiasis, because it created many new habitats for the snails that are the intermediate hosts for the parasitic worm that causes this disease. A lot of people became exposed to the infection, and there was an increase in schistosomiasis.

Now, that wasn't the purpose of the Aswan Dam, obviously. It was to produce hydroelectric power, which it was partially successful at. If I understand correctly, some of the generators that the Russians put in there still haven't operated. The lake is beginning to silt up behind the dam, so it won't be functional at some future date. Agriculture has not necessarily been improved by the change, because they no longer get the siltation in the Nile Valley which was a major factor adding to the fertility of that land. The dam supposedly also has had adverse effects on the fisheries in the Mediterranean and many other things, but that's not my area of expertise. The World Bank did not ask the society to continue to be a consultant on the problem, but in retrospect some volunteers such as Dr. Heyneman have given it to them for free.

Hughes: Do you think they consulted with anybody who would know about the diseases they were likely to encounter?

Reeves: I'm sure they have. Not necessarily because they asked for it, but because people probably volunteered it to them and it was important to do so.



**President, American Society of Tropical Medicine and Hygiene,  
1970-1971**

Hughes: Any more comments on your years as president of the American Society of Tropical Medicine and Hygiene?

Reeves: Yes, there is one thing you have to realize. A president's term in one of these societies is for one year. If you get something off the ground in that year, you're fortunate. But at the same time, you have no control whatsoever over who your successor is going to be or whether his interest is going to be the same. So societies frequently do not do follow-up on projects they initiate.

The one time that the president has an opportunity to really influence the membership is when he gives the presidential address just before the annual business meeting--that is, when the challenge of his major concerns can be thrown out. The title of my address in 1971 was, "Can the war to contain infectious diseases be lost?" I predicted it could be, and many subsequent presidents have referred to that paper and presented specific examples.

Hughes: Was there any sort of initiation process when you became president?

Reeves: None. The only initiation that I had was having been on the council. I knew that the executive sessions of the council were far too long and sometimes unproductive. They didn't have an agenda with a time limit, the meetings usually were held so that they ran way into the night, and everybody was too tired to work effectively. If I did one thing during my tenure besides the World Bank thing and my presidential address, it was to recast the general protocol for the executive council meetings so that it didn't meet during the scientific meetings, at night, and at all sorts of odd hours. The executive council now meets before the society meetings start, considers the basic agenda and what they have made in the way of decisions, and what advice they need from the membership. So at the business meeting the actions of the council can be thrown out for review by the total membership and be subject to resolutions from the floor. I also worked with the secretary, George R. Healy, and put some time limits on the length of the executive sessions and the way in which material was organized beforehand. So I tried to do something to streamline or increase the efficiency of the executive council meeting.

Hughes: Were your changes carried on?

Reeves: Yes, the executive council meetings are now held before the scientific meetings, and then at the end they have another executive council meeting which includes bringing the new members onto the council.

Hughes: Does the council consider it within its purview to make pronouncements on issues related to tropical medicine?

Reeves: The council as such usually doesn't. It depends upon issues being put on the floor by and to the membership as possible items of concern, and then resolutions are made by the membership from the floor. The feeling is that a resolution from the membership, even though it instructs the council, is probably more powerful than one that the council does by itself. But I don't know. My last direct involvements with the council from an administrative viewpoint were the years on the council and a year as its president. After that I've never been to a council meeting.

Hughes: Is it mandatory to serve on the council?

Reeves: No. A person does not have to have served on the council to become the president. They usually have, but not always. There is a president-elect who attends the council meetings the year before he takes over. Today the president-elect becomes very much involved in the organization of the program for the annual meeting the following year. He's not the program chairman but becomes involved in it and attends the council meetings.

Hughes: But you didn't do that?

Reeves: I don't remember being president-elect; I went from being on the council to being the president. The presidency of a society is looked upon as an honor, and everybody recognizes the lack of tenure in the position. I think many people overrate what a president can accomplish.

Hughes: Who was around on a permanent basis to keep the society moving?

Reeves: They have a secretary, a treasurer, the continuing and new members of the council, and the new president. They have received charges from the membership during the annual business meeting or by mail ballot.

Hughes: But those are annual appointments too, aren't they?

Reeves: No, council members are for two years, and the president-elect serves a year on the council and is then president. The editor of the journal, the secretary, and the treasurer are continuous for a

period of years. The American Society of Tropical Medicine has now hired a professional group to handle the bookkeeping--what amounts to the duties of a treasurer--so much of the administrative aspect of the society is run by a professional group and not on a voluntary unpaid basis. The society employs a person or persons to handle affairs on the Washington, D.C., scene.

Hughes: When did that change come about?

Reeves: That came when Karl Johnson was the president in 1986-1987. One of his major moves as the president was reorganization of the society's administrative activities. He also made a rather major change by involving the organization in a much more serious fashion in getting the needs of tropical medicine recognized nationally by politicians. He initiated it in such a way that it has been a continuing objective of the three presidents who followed him. He presented it in such a forceful fashion that they had no choice. They have accepted the fact that the organization has to be involved in trying to influence the politicians, that there is a continuing real need for research and program funding by the United States Government in recognition of the importance of tropical diseases to our country and internationally.

#### Political Action in Washington, D.C.

Hughes: Does the society have a lobbyist?

Reeves: For practical purposes, yes. It has a staff in Washington which deals with the political actions that impact on tropical medicine, so they actually function as lobbyists. Whether they're registered lobbyists or not, I do not know. Many organizations have offices in Washington that are very busy knowing what bills are on the floor, influencing bills going in, making appointments for people to see the important people on the congressional committees, letting the membership of the society know to whom letters should be written. But it's somewhat different from a commercial organization hiring a lobbyist. The members of the society who become involved certainly are not licensed and registered lobbyists, and the members who are employed by the federal government must be very careful in how they become involved.

Hughes: Is this pretty true of all the organizations that you know about?



Reeves: It's certainly true of the medical and public health societies.

This is the other thing that Karl [Johnson] recognized: you cannot do effective political interaction at the Washington level with a bunch of amateurs coming in from the outside on a periodic basis and not really knowing whom to see about what. It's a professional job, and it's a big job. So the society members had to be convinced that they had to increase dues and make contributions to support such political activity. It's a very expensive process.

Hughes: But a critical one, I would think.

Reeves: Very critical, and sometimes discouragingly ineffective. You expect, if you go in there and contact all these people, that they're intelligent human beings and are going to do what you think is important. Frequently, five years later you find it's amazing how little you've actually been able to accomplish. I would say such action is a big agenda for the society now, whereas the effort we made with the World Bank was sort of chicken feed. Some of us went in as citizens representing our society to talk to these people, and they fortunately listened to us.

Hughes: But that was the way things were done in the 1970s and before?

Reeves: Yes.

Hughes: Most organizations didn't have an elaborate structure for political action?

Reeves: No, but the AMA did at that early time, and the American Public Health Association had a full-time professional staff taking care of political issues in Washington. So did the university; the university has offices in Washington and in Sacramento. They're not registered lobbyists, but they're certainly there to communicate concerns and needs of the university. When I was dean of the school, I frequently used the university office in Washington, D.C., and in Sacramento to make appointments. But what I was doing in large part in D.C. was to represent the Association of the Schools of Public Health or some society; I wasn't only representing our university. The dean from each region was responsible for making the political contacts in Washington for his geographical region for the Association of Schools of Public Health. When I was dean, I had to contact the representatives from Montana, Idaho, and all the other western states that didn't have a school of public health, as well as the California legislators and senators.

Hughes: Did you have people in Congress whose ear you could obtain?

Reeves: When I was dean I had no difficulty getting hold of [Senator Alan] Cranston and other representatives at their offices. I also found that if I got on a certain flight on a Friday evening, Senator Cranston would be coming back to the West Coast. The flight always stopped in Kansas City; it was the easiest time to meet him.

Hughes: And you actually talked to him in person?

Reeves: He knew who I was, sure. When he'd get off to do his jogging in Kansas City, I'd say hello to him when he left the plane and watch him run around the airport. When he came back, I'd go ask him if he had a few minutes when I could talk to him. He didn't mind. He was in the tourist class along with the rest of us.

Hughes: Good for Cranston.

Reeves: He always had the bulkhead seat. [laughter] Bob Goldsmith was the university representative in Washington, D.C., and he'd been the student body president during one of the first big student demonstrations on the Berkeley campus. Bob was the head of our office in Washington. He had a good staff and a very effective personality. He had made contact with all the critical people in Washington and opened their doors; they all knew him. He'd formed a Cal Alumni Association branch in Washington, D.C. The administrative assistants in many senators' and congressmen's offices were graduates of the Berkeley campus and other University of California campuses, so he had a big alumni group that he worked with.

To come back to the Society of Tropical Medicine, each year the president and his staff now have a breakfast for all the past presidents of the society who are at the meeting. At that time they review the present problems of the society and ask for any suggestions the past presidents may have for action. Or they may pinpoint a past president who is in a position to make a contact with someone in the government who is particularly critical politically at that time. So they use the past presidents as sort of an "old boys" club that goes all the way back to when the society had its origin.

Originally there were two societies: the American Society of Tropical Medicine and Hygiene, and the American Society of Malariology. It must have been in the forties that the two societies decided to become one society. Past presidents from both societies still show up at these breakfasts.

The only other group that now really overlaps significantly in interests is the American Society of Parasitology. In recognition of that, the two societies meet for a joint meeting every five years. That's a big turnout and a big program when they do.

Hughes: You're not a member, are you?

Reeves: I'm not a member of the American Society of Parasitology because that deals with protozoa and worms, and I've never had that much of an interest in the larger parasitic organisms.

The other thing the society does is to periodically have a joint meeting with the Royal Society of Tropical Medicine from England. They come over to the U.S.A. and have a meeting with us. As far as I know, our society has never had a joint meeting with them in England.

#### The Historical Archives of the American Society of Tropical Medicine and Hygiene##

Hughes: Let's get back to and finish activities of the ASTMH.

Reeves: There is one more thing on the American Society of Tropical Medicine which I think is relevant. I think this organization is almost unique because it has developed its own archives, which Dr. Linda Brink is in charge of. The archives are housed in the library at Harvard University. They're accumulating historical material which is relevant to developments in tropical medicine in the United States. This has been an amazingly effective effort and has resulted in a large collection of photographs and documents of various types. In addition, they have developed a library of videotapes of selected individuals.<sup>1</sup> These persons are interviewed and put on videotape. A fifty-minute to one-hour video is then developed on that person's career. In many ways it's similar to the oral history which we're doing now, except that it is visual and audio. Originally they selected older investigators in the field to tape. They usually had a younger or contemporary investigator do the interview. That way, many of the young people in tropical medicine got good coverage as well as the person who was being interviewed. More recently they have done interviews of people who are at mid-career and who have made outstanding contributions to tropical medicine.

Hughes: Whose idea was it to do this?

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<sup>1</sup>The series includes: Jordi Casals, Calista Causey, Robert Coatney, Wilbur Downs, Donald Hopkins, Harry Hoogstraal, Austin Kerr, Thomas Monath, Ruth Nussenzweig, Jose Oliver-Gonzalez, William Reeves, Paul Russell, Philip Russell, Albert Sabin, William Sodeman, Sr., James Steele, Lucy Taliaferro, Thomas Weller, Telford Work, and Martin Young.



Reeves: I don't know. It may have been Linda Brink's, who organizes and edits each tape. The project was endorsed and funded by the council of the society. It largely started with arbovirologists, for whatever reason--probably because arbovirology has been a very active part of the society, but the film series also has included parasitologists and other people. Linda Brink had extensive training that made her a very competent person to do this. She has a Ph.D. in parasitology and has made training films for teaching. She's very good at talking people into giving financial support or use of facilities and staff to do the filming, either in professional studios or at federal agencies like the Walter Reed Army Institute of Research or the CDC in Atlanta, Georgia. When I was filmed by her and interviewed by Dr. Karl Johnson, we did it in a private studio in Los Angeles. The owner of the studio was interested in using television for educational purposes, and he just donated his time and staff. We also did it there because the tropical medicine meetings were in Los Angeles that year. The interview required almost three hours and when completed was edited to a fifty-minute presentation. I would say that Linda has now completed thirty or more films in this series.

Hughes: Wonderful.

#### Awards Given by Societies

Reeves: The ACAV has a series of awards that they make every several years, one of which is the R. M. Taylor Award for outstanding contributions in arbovirology. The first recipient was Dr. Taylor himself, who was in the Rockefeller Foundation and was the first editor of The International Catalog of Arboviruses. He'd made many contributions internationally in arbovirology. They give that award every third year now. So many of those who were the initial people in this field have gotten the award--Drs. Casals, Hammon, Taylor, and myself.'

The Committee on Medical Entomology of the American Society of Tropical Medicine and Hygiene established the Harry Hoogstraal Award. Harry was a very good medical entomologist and a very interesting person who, for practical purposes, spent most of his

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'R. M. Taylor Awards: R. M. Taylor, 1966; J. Casals, 1968; W. McD. Hammon, 1970; W. C. Reeves, 1973; R. W. Chamberlain, 1975; P. Galindo, 1977; W. G. Downs, 1977; Otis and Calista Causey, 1980; T. H. Work, 1981; T. H. G. Aitken, 1984; H. Hoogstraal, 1984; R. E. Shope, 1987; K. M. Johnson, 1987; J. L. Hardy, 1990.

career after World War II at the Navy Medical Research Unit in Cairo, Egypt. He was the world's expert on ticks and tick-borne diseases and used the Cairo laboratory as a base for extensive scientific work, not just in Egypt but elsewhere. He died a few years ago, and they established the award in his name. The award was for outstanding accomplishments in medical entomology, and they've given it out, I guess, three or four times now.

Hughes: Have you gotten that one?

Reeves: I was the first recipient, in 1987.

Hughes: Do you know why?

Reeves: I don't know. Maybe they started with the oldest people in the field and will work down eventually.

Hughes: Was there a citation?

Reeves: Yes, there's a medal and a citation that goes with it. The American Society of Tropical Medicine has a series of awards that they give out. The highest award is the Walter Reed Medal, which is in honor of Walter Reed's landmark research on mosquitoes as the vectors of yellow fever. That work was done in 1900 in Cuba. They give this award every three years or five years. I felt very honored to get it in 1987. I have been very fortunate and honored to receive several of the society awards as well as awards from other societies and associations.<sup>1</sup>

#### The American Committee on Medical Entomology

Hughes: What about the committee on medical entomology?

Reeves: There are two committees which are appendices to the American Society of Tropical Medicine and Hygiene. The original one was the American Committee on Arthropod-Borne Viruses [ACAV], which I talked about earlier. We're not a separate incorporated organization; we're part of the American Society of Tropical Medicine and Hygiene. Probably five years ago, the medical

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<sup>1</sup>American Public Health Association, John Snow Award; Delta Omega Society, National Merit Award; American Veterinary Epidemiology Society, Dr. K. F. Meyer Gold Headed Cane; American Mosquito Control Association, Medal of Honor; California Mosquito and Vector Control Association, Meritorious Service Award.

entomologists in the society were looking for a way in which they could have a half-day meeting, and they took the ACAV as a model. They developed a similar organization, namely a bunch of volunteers interested in medical entomology and concerned about the future of their field. It is now associated with the society, as is the American Committee for Medical Entomology (ACME).

### Manpower Needs in Entomology##

Hughes: I see that you recently wrote an editorial on manpower needs in medical entomology.<sup>1</sup> What is the problem?

Reeves: Yes, many of us believe there is a need to take steps to assure there are adequate competent people in medical entomology in the future. The American Society of Tropical Medicine and Hygiene and ACME believed this was a real problem; perhaps a crisis would be a better statement. The Medical and Veterinary Section of the Entomological Society of America also had recognized the problem. Earlier, in 1983, the National Research Council had called a workshop to consider this problem and issued a report.<sup>2</sup> This same organization issued a second report<sup>3</sup> in 1987 that addressed the problem on a broader basis, and it still specifically focused attention on medical entomology as one central area of concern. Yet the problem continued in 1990-1991.

I will try to summarize the problem. The training base in medical entomology has been dramatically decreased. Courses in this field have been abolished or markedly decreased in the curricula of many universities. As academic positions in medical entomology have been vacated by retirements, many have been transferred to other fields. The time when medical entomology is really a dire need nationally is during wars. Many of the

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<sup>1</sup>Reeves, W. C., Concerns about the future of medical entomology in tropical medicine research. Amer. J. Trop. Med. Hyg 40: 569-570, 1989.

<sup>2</sup>Manpower Needs and Areas of Opportunities in the Field Aspects of Vector Biology, Board on Science and Technology for International Development. National Research Council, 1983.

<sup>3</sup>The U.S. Capacity to Address Tropical Infectious Disease Problems. Board on Science and Technology for International Development, Office of International Affairs. National Research Council and the Institute of Medicine, National Academy of Sciences, 1987. National Academy Press, Wash., D.C., 1987. p. 1-172.



arthropod-borne diseases--malaria, typhus, plague, and others--become very important during wars because they are a major cause of casualties among soldiers who are exposed; and civilian populations are disrupted, so they don't have the benefits of vector control programs to protect them. Indeed, the outcome of most wars has been decided by the occurrence of arthropod-transmitted and other infectious diseases that are associated with war, poverty, uncleanness, and so on. Whenever there's a war, there's a dire need for a lot of medical entomologists who understand vector-borne diseases to carry out vector control and to do research on unsolved problems of newly-emerging diseases. Between wars, there's a big lull in which the control programs for vector-borne diseases are more national or local in the sense that they are concerns of the Public Health Service or state health departments or local communities. There's less of a demand for large numbers of entomologists to work overseas to protect nonimmune people who are now in underdeveloped areas of the world for the first time being exposed to these diseases.

Today's medical entomologists--that is, the American Committee on Medical Entomology and the Medical and Veterinary Section of the Entomological Society of America--became very concerned with the future of medical entomology in the United States. There has not been a revision of a textbook in medical entomology or a new one published in probably the last fifteen to twenty years, and the field has had many, many developments in that time period. If a new book is written, the prospective publisher has to think twice about who the audience is going to be. Will it sell and make a profit? Most of the curricula in medical entomology in universities and colleges have been abolished, and there's a decrease in the number of jobs in this field. The number of readers has decreased, but it doesn't mean there isn't still a need for a good textbook or continued research. It's just a matter of whether the politics and policies of different agencies that hire entomologists are going to provide a job market for medical entomologists. It's a matter of whether our government will invest significant funds for research and control programs on major tropical diseases in underdeveloped areas of the world during so-called normal times.

The American Committee on Medical Entomology and the American Committee on Arthropod-Borne Viruses now each have a half-day program at the tropical medicine meetings. In addition, they're allowed to play a major part in the program development of other parts of the four- or five-day program that the society has that gives these fields visibility and a platform from which to present their research contributions and concerns for the future of their fields.

Hughes: We've spoken already of the problem of emerging viruses in your work with the National Academy of Sciences. Do you think that one of the results of these meetings will be a sustained effort in medical entomology and tropical diseases?

Reeves: I certainly hope so. I already referred to earlier meetings and reports on manpower needs in medical entomology and parasitology that were held under the auspices of the National Academy of Sciences.

Hughes: Were those reports written in response to the problem of the emerging viruses?

Reeves: No. They were in response to the general feeling that medical entomology and parasitology were scientific fields in which the United States was losing its leadership in training and research and that there was going to be a continuing need for people in these areas. It was felt that this problem should be brought to public and political attention.

Now, those earlier reports and current concerns are being referred to by the new committee on microbial threats to health that the Institute of Medicine of the National Academy of Sciences has. One of the problems will be to determine to what degree the diseases transmitted by arthropods or caused by parasitic infections are going to be considered as a significant part of the emerging disease problem. I'm quite confident that when the final report comes out from the academy, considerable attention will be given to such diseases.

Hughes: Isn't your very presence on the committee an indication that it is considering the problem of arthropod-borne viruses?

Reeves: I would assume so, and I'm doing my best to make sure that's the case. There are four subcommittees which are considering a wide array of problems. Drs. J. L. Hardy and B. F. Eldridge from our university are on a subcommittee which concerned itself with the needs for medical entomology workers and research in this field. Dr. Fred Murphy from the Centers for Disease Control, who is the new dean of the veterinary school in Davis as of August [1991], also was on that subcommittee, and most of his career has been spent studying arthropod-borne diseases and hemorrhagic fevers.



### Problems Related to Control of Infectious Diseases

Reeves: I'm sure the needs of medical entomology and arthropod-borne diseases are going to come out. I can't see how you can talk about emerging diseases that are likely to become problems without considering the large group of over five hundred viral agents that have arthropods as vectors. We've already learned their importance historically, and they're going to continue to be problems because of the problems we have in trying to control them, the resistance of vectors to insecticides, and the lack of vaccines adequate to protect against them. There's a big field that still must be studied and improvements made for their detection and control. We are going to continue to have epidemics of such diseases.

Hughes: Why are vaccines so hard to come by?

Reeves: One of the problems with vaccines is the legal restrictions that are put on vaccines before they can be made, licensed, and released for use. Also, a lot of the diseases that we are discussing are very sporadic. In other words, you can't necessarily say that there is going to be a big epidemic of disease A or B next year. For vaccines to be approved you have to demonstrate their efficacy before they can be licensed. That means that you have to have a situation like we had for the polio vaccines: before it would be licensed, you had to have a field demonstration that the polio vaccine would protect against the disease, which meant a placebo study with controls that didn't receive vaccine and other people who received the vaccine. It was a double-blind study, so nobody knew who had what until the field test was over. Well, can you imagine doing a vaccine study on one of the viruses that we work with? These are infections of nature transmissible to man. For many infections, people are of no importance in their maintenance. You might go out and vaccinate a big population, and the disease would not appear that year; but the disease agent would still persist in its animal reservoirs and vectors. If an epidemic is already happening, it's too late to go in and evaluate a vaccine. In addition, the disease will only occur in the summer, so you can't get the vaccine in early enough for evaluation. So that's one problem.

Dengue fever is a good example of a second problem. Dengue fever is a disease caused by four distinct viruses. Any of the four can cause dengue fever and the hemorrhagic fever/shock syndrome associated with it. That means you don't have to have a vaccine for one virus; you have to have four vaccines, and preferably the four must be combined into one. This becomes very involved technically, economically, and medically. One hypothesis



is that the hemorrhagic fever/shock syndrome is an immunological type disease that requires sequential infection with more than one type of dengue virus. If this is true, you need to have a vaccine for all four types to be given simultaneously. The other hypothesis is that the virulence of any one dengue type can change or it can produce the same disease. You still need a vaccine to cover all four virus types.

Then the final problem is whether the people who live in places where the disease occurs can afford a vaccine. This is a problem you get into whether it's a hepatitis, typhoid, cholera, or malaria vaccine. If that vaccine is produced using molecular biology approaches, it may be a very expensive product. The country which needs it the most doesn't have the money to produce it or to pay for it. You want a vaccine that costs twenty-five to fifty cents per person. If an American company is going to make a living out of making that vaccine, it may have to charge five, ten, fifteen, or twenty-five dollars for the vaccine per person.

Hughes: So the problem is not the inability to develop an effective vaccine?

Reeves: It can be both an inability to produce and the final price. I think the problems they've run into with the malaria vaccines are another example. Ten years ago the technical knowledge to develop a malaria vaccine was believed to be there, but it remained to be seen whether it would be effective. The technical knowledge might be very good, but maybe we didn't know enough about the malaria parasites. Perhaps we didn't know how much they would vary genetically and get around the vaccine.

The malaria parasite, like many other infectious agents, has already demonstrated an amazing ability to develop resistance to drugs. We're almost back to where we started from, where quinine could become the only effective drug we have for malaria.

Hughes: Why doesn't the parasite develop resistance to quinine?

Reeves: God only knows, and he hasn't told us yet. If we had God as our consultant, we could do a lot better in answering some of these questions. Some of the antimalarial drugs in some ways mimic the chemical structure of quinine. The basic problem is whether we have enough knowledge to control the disease by a method that would be economically acceptable, feasible, and effective enough to permanently stop the disease. The objective may still require vector control. Insecticides can achieve a great deal of control, but insecticides also become expensive, and the vector develops resistance to them so that you can't use them any more. The WHO had a worldwide program to control, even eradicate, malaria based

on DDT. It didn't accomplish that objective for a wide variety of reasons. In addition, in the developed world--and the United States is a prime example--if there's proof or even a hint that a particular insecticide or drug might have adverse effects on the health of man or other animals, you are told you can't use it; it will not be licensed and cannot be sold. That's what I call the do-gooder's influence, but I'm not using the term "do-gooders" in the sense of criticism. These are the people who feel that if there is any possible way in which something can be harmful to them or their environment, we shouldn't be using it. They may or may not be right, but they have a lot of power.

Hughes: Is Rachel Carson the source of all that?

Reeves: Certainly not of all of it, but she gave it a head start with her book, Silent Spring. When she wrote that book, she didn't have to have proof of everything she was saying, because she wasn't responsible to any agency of government or to any individual. I'm not saying she was all wrong, but a lot of her statements were not necessarily based in fact. But it's not my purpose to get into that sort of controversy.

Many infectious agents are very capable of changing genetically to get around the human body's ability to produce antibodies to them and thereby prevent their multiplication. Insects by nature are capable of rapid genetic changes that will protect them from toxic agents. Influenza is a classic example of change in an infectious agent. When the influenza virus comes up against an antibody, its antigenic makeup begins to modify to get around that antibody. This can happen very rapidly, and whether it occurs in man or another animal host is immaterial. It happens. Vectors also are very capable of biological change, so we are in an arena of constantly changing factors. I have oversimplified my explanation but I hope sufficient for our purpose.

As regulations get more and more stringent on licensing a drug or an insecticide, the costs rise. Pretty soon the market can't stand the cost. If the commercial companies in the United States--or Britain, Germany, or wherever they are--can't make a living out of developing and marketing drugs or insecticides, they will stop their efforts. They are not out there to be do-gooders just for human benefit; they're out there to make enough money to at least survive and support their operation.

Hughes: That applies to manufacturers of insecticides as well as to the drug companies?



Reeves: Absolutely. That's why the control program for vector-borne diseases has become almost completely dependent upon the agricultural need for insecticides for agricultural pests. New insecticides are not being synthesized and manufactured for vector control.

**International Northwest Conference on Diseases of Nature  
Communicable to Man**

Hughes: Well, shall we talk about the INKY DINK Society?

Reeves: Yes, the International Northwest Conference on Diseases of Nature Communicable to Man, with its great abbreviation, the INCDCNM, which some call the INKY DINK or INCUM-DINKUM. It's one of the most interesting organizations I know of. It's not incorporated, it doesn't have annual dues, it meets annually. It represents a group of people in Canada and the United States, and that's why we call it international.

The primary leading force that started this organization was the staff of the Rocky Mountain Lab in Hamilton, Montana. In 1946, they saw a need to get people together from the western area of North America who were concerned with plague, Rocky Mountain spotted fever, Q fever, relapsing fever, and the encephalitides. These are diseases of nature, and that's where the name of the group came from. Each disease has its basic cycle in wildlife and then gets transmitted incidentally to people by an arthropod. There's a thread that goes through: Rocky Mountain spotted fever with ticks, relapsing fever with ticks, Lyme disease with ticks, western and St. Louis encephalitis with mosquitoes, plague with fleas, and so on down the line. So any disease that has its base in a natural cycle in wildlife, usually with a vector involved but not exclusively, is of interest to this group. The most recent addition is Lyme disease. They thought if such people got together every year just to talk--nothing formal, no journal, as no publication comes out of this that is referable--these people would talk about what they were doing currently, get it out in the open, get collaboration between people, and so on.

Hughes: Were you involved with the foundation of the group?

Reeves: I wasn't, but I came into it very soon after it was formed in 1949. I was invited to give a presentation at one of their meetings, which was held in Berkeley in 1949.



This group goes at its own pace. It meets each year at a different place, usually where there are student dormitories available for housing, so they keep the cost very low. There are no dues. They always meet in August, and that means people can bring their kids. My entire family enjoyed an earlier meeting at Ashland, Oregon, as it included going to their Shakespeare theater.

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Reeves: They do put out a summary of what goes on at their annual meeting. These proceedings are sent to participants, but they're not a publication, and the proceedings are marked, "Not for publication. Not to be used as a reference." So if you participate, you get some new information and make new friends.

They have a memorial lecture, the R. R. Parker Memorial Lecture. Dr. Parker was one of the early directors of the Rocky Mountain laboratory in Hamilton, Montana, which is a National Institute of Allergy and Infectious Diseases laboratory. The laboratory was established in Montana because that's where Rocky Mountain spotted fever was originally recognized as a problem. Every five or six years INKY DINK meets up there, and every third year they meet in Canada. The meetings are informal. If somebody suddenly decides they want to get up and talk, they do so. In some ways it's almost like a revival meeting. They're fun to go to.

In 1969 I found out I'd been elected president. That was a year they met up in Vancouver, Canada. After that meeting, for some years I didn't go to them, because their meetings in August are at a most improper time for me, as arboviruses are very active at that time, and we're in the middle of our field research. I was invited several years ago to be the Parker lecturer when they met in Calgary, Canada.<sup>1</sup> I'm going again this year to the forty-sixth meeting, as they're going to meet at Sonoma State College. In 1991 I talked on the potential impact of global warming on encephalitis viruses and their vectors.

Hughes: What disciplines are represented?

Reeves: No disciplines are represented as such, but there are a lot of field ecology/epidemiology sorts of people such as myself. That's

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<sup>1</sup>W. C. Reeves. Delights and delusions experienced in fifty years of arbovirus research. 1989. R. R. Parker Address, 44th annual meeting of International Northwest Conference on Diseases in Nature Communicable to Man. Calgary, Canada, Aug. 12-16, 1989.

not exactly a discipline, but there are people who are interested in looking at diseases that are out there in nature--in the birds, in the mammals, in the ticks, in the fleas. There are physicians, veterinarians, entomologists, vertebrate ecologists, laboratory microbiologists; many disciplines are represented. Lyme disease is a current focus of considerable interest to this group. So Dr. Robert Lane, David and Evelyne Lennette, and Bob Murray from California are some of the main people interested in the current meetings.

The members of INKY DINK enjoy going to something that isn't a big national meeting with two thousand people. There's never been a commercial exhibit at these meetings, because there's no market represented. The meetings don't draw people who have big budgets to buy a ton of insecticide or endless drugs to treat a lot of cases.

Hughes: What about young people coming into the field?

Reeves: A serious effort is made to bring new and younger people in as fast as they can. If a new person comes into one of the laboratories where an older person who is a member is involved, he gets that person to go to the meetings.

Hughes: Dr. Harald Johnson told me that there was a struggle to get young people into fieldwork.<sup>1</sup> They'd much rather sit in a lab and look down a microscope.

Reeves: I'm not sure they'd much rather do that, but it may be the only place they can get a job. But some people come who do nothing but sit and look in the microscope or administer programs. Drs. Emmons and E. H. Lennette have been very active in this group, and they hardly ever get out in the field but would like to. Their jobs don't permit it. They are still interested in this area and show up at the meetings. Dr. Lennette hasn't done any fieldwork for half a century. Excuse me. [laughter] Not since his Q fever work.<sup>2</sup> But he got interested in this organization when he was interested in Q fever, and on occasion he was out wrestling sheep and sampling them. Q fever was a very conspicuous disease in the early meetings of this organization. It's a disease that's in sheep and other animals like that, and it accidentally infects man. Man is not important in maintaining the organism. So all these oddball diseases are concerns of the

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<sup>1</sup>See the oral history of Harald N. Johnson.

<sup>2</sup>See oral history with Edwin H. Lennette for a discussion of his Q fever work.

society. Dr. Lennette was president of INKY DINK several years ago when they met at Humboldt State College.

They don't give any awards, don't have any big national meetings, don't have a published proceedings or a journal, and no dues. I can only conclude that a little informality doesn't hurt anybody occasionally. I think this had a possible influence on me in the formation of the ACAV, because the ACAV again has been as informal as we can keep it. We're not incorporated as a group and don't pay dues.

Hughes: Do you think that was a conscious association?

Reeves: No, but I'm sure that it influenced me.

#### American Public Health Association

Hughes: Well, shall we move on to the American Public Health Association?

Reeves: Yes. The American Public Health Association (APHA) is concerned with the formal public health aspects of our society. The membership is primarily made up of professionals and represents people who work in federal, state, and local health agencies. The organization, when I first joined it in the early forties, was very much like the tropical medicine society in that the purpose was to present serious scientific papers. The American Journal of Public Health was a very important outlet for scientific papers, and you gave presentations at the annual meetings because that increased the possibility that your findings would be published in that journal. This was true during the 1940s and 1950s when I was most involved with the APHA.

Now it's so large an organization that it is divided into ten or twelve subgroups. Their national meetings are huge; there are thousands of people there.

Hughes: You were associated with the epidemiology section?

Reeves: I was involved in the formation of the epidemiology section. There were a lot of epidemiologists in the APHA, but we didn't have a section where we could meet, so we started a move to establish one. They have rules and regulations of how many people have to be represented before there can be a section. You had to have a critical number, say a hundred or something like that.



So we formed a section, and it soon blossomed out, because it incorporated interests and research on both infectious and chronic disease epidemiology. Everybody pays lip service to the fact that if we don't know the epidemiology of a disease, we don't understand its public health importance or how to control it. So it became a rather important section with a large attendance. The environmental health, maternal and child health, and health officer sections had to turn to us for information, the people concerned with diseases of the elderly had to turn to us, et cetera, et cetera. It became a very active and productive section. Important and usually critical papers in epidemiology were published in that journal.

Hughes: You were a council member and past secretary of the epidemiology section?

Reeves: Yes. When the section was formed, Dr. Robert Dyar from the California State Health Department was the first chairman, and I was secretary. We knew each other, so we worked together very well. When the epidemiology section was recognized, the officers also became members of the council of the APHA.

Reeves: The first time I went to an American Public Health Association meeting was in 1942. Dr. Hammon and I were invited to come to the meeting that was held in St. Louis to report to the health officer section on our findings on St. Louis and western encephalitis in the Yakima Valley. We got on the train to St. Louis, taking a typewriter with us, had a compartment, and sat and pounded out the paper. This gave us a couple of days to do it, and it was the first free time we'd had. We took our data with us, and it was published in the APHA journal.<sup>1</sup> It was an exciting paper and place to present it. St. Louis had been the site of the first St. Louis encephalitis epidemic in 1933, and our paper unequivocally demonstrated for the first time that it was mosquito-borne and that mosquito control was the approach for prevention.

In the mid-1960s things really began to change in the APHA, which had never really been politically oriented. They then became very much involved in the politics of public health at a national level and extremely concerned with the problems of minority groups and social rights, to the extent that they openly said that a significant part of the annual meeting was going to be devoted to these political issues. Some activist groups

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<sup>1</sup>W. McD. Hammon, W. C. Reeves, M. Gray. 1943. Mosquito vectors and inapparent animal reservoirs of St. Louis and western equine encephalitis viruses. *AJPH* 33:201-107.

recognized the direction APHA was taking, so they came to the annual meeting to protest anything they wanted to. As a result, there was a significant drop-off in program content that had anything to do with scientific research. It became much more political.

Hughes: Is this characteristic of public health people?

Reeves: I think the whole public health movement has moved very strongly in that direction. Some of the most effective lobbyists for public health movements are the activist groups. I respect what they're doing, and I respect their interest, but when I was told to my face by some people that you don't come to these meetings primarily for science purposes, I couldn't take that. As a matter of fact, I could even say I couldn't stomach it, because the liberals were not being permissive enough about the things that many of us were still interested in. As a matter of fact, the first permanent secretary they had for the APHA had been a student here in the school and was the first person hired as its executive secretary. He was very open about the changes. This was his interest, and this was the way things were going to be. I prefer not to identify him by name.

Well, they even planned at one time to put what I would consider to be a means test on your dues. If you were a high-salaried person, you paid higher dues than a lower-salaried person. The object was to raise more money for activist types of programs. I objected to that and withdrew my personal membership. It happened that I was dean of our school at that time. The secretary of the organization called me and said I couldn't withdraw my membership; I was dean of the school. He sent people to work on me to not drop my membership. They said you couldn't have a dean of a school of public health not be a member. I finally became exasperated and said, "I can do better than withdraw my membership. I can drop the school from its associate membership in the APHA." Well, fortunately he believed me. He stopped heckling me, and I didn't drop the school's membership. I really didn't want to, as I believe there should be an APHA.

Hughes: Could you have?

Reeves: Hell, yes, I could have. All I had to do was not sign the check for the membership. [laughs]

I've been to one American Public Health Association meeting since then. They were meeting in Toronto, Canada, in 1982. I got a telephone call from Dr. Ralph Paffenberger, who at that time was the chairman of the epidemiology section, and he was very excited. "Bill," he says, "we've selected you to get the John Snow Award

from the Epidemiology Section of the American Public Health Association. Are you going to be at the APHA meeting in Toronto?" I said, "I wasn't planning on it. I'm not a member of the association." He said, "What?" I said, "I'm not a member of the APHA anymore. I haven't been for some years.. Now, look, if that makes any difference, don't give me the award. If you have to be a member to receive it, I understand that. Just don't give it to me." To which he responded, "Oh, no, no, it won't make any difference." So I said, "I'm going to be in Michigan just before the Toronto meetings, so I'll be glad to come on up there. However, you have to get me into the meetings." He replied, "Well, that won't be any problem." I said, "I'm not too sure of that."

A few days later his secretary called: "Well, we have a little problem here. There's a registration fee for nonmembers," and it was \$50 or something. I said, "That's exorbitant. I'm not going to pay it." [laughs] Anyway, to make a long story short, it finally was set up that he was given a pass for me, but he had to meet me at the outside door and escort me in. I had a great big red badge that said, "Nonmember," and it limited to one half-day the time that I could be there. I wore it with pride.

So I went in, had a very nice luncheon, and got the John Snow Award, which I was very happy to get. John Snow's studies on cholera in London are one of the classics in epidemiology. As I've mentioned, I spend a lot of time with students discussing his contributions to the field. It's an award that meant a lot to me. It's like getting the Walter Reed Medal from the American Society of Tropical Medicine. Those are the sorts of awards that really mean something to you and that you really like, as they represent recognition by your peers in your field.

I still do not go to the American Public Health Association meetings, and it's primarily because their political focus of interest doesn't appeal to me. I have plenty of other chances to get into political activities if I want to.

#### **American Epidemiological Society**

Hughes: The American Epidemiological Society.

Reeves: This organization was formed in the 1930s. Originally it represented a close-knit clique from the eastern United States, where most of the basic epidemiology was being done--Harvard, Yale, Johns Hopkins, and those places.



Hughes: Mainly because that's where the schools of public health were?

Reeves: That's where the schools of public health were and where the field of epidemiology had evolved in the United States. In the thirties we just had the Department of Hygiene at Berkeley and a small state health department. It wasn't until the forties that we had a school. By the mid-1940s, people on the eastern seaboard were recognizing that there were people and activities in epidemiology in the western United States. However, very few people in the western United States had been elected to membership in the American Epidemiological Society. Dr. K. F. Meyer and Dr. W. McD. Hammon had been elected members.

The society bylaws strictly limited the number of members. In 1950 the bylaws of the society imposed a limit of a hundred members. If a member didn't die or resign, no new member was elected; so one or two people might be elected per year. They still have a rule that you have to go to a meeting at least every three years or you are dropped from the membership. The meetings were generally held in the eastern United States. I wasn't a member when the initial suggestion was made to meet in California. That reaction was interesting. It would be like going overseas. In the 1940s it was cheaper and easier to go to Europe than it was to go to California. Anyway, as you know, anyone from New England is a little provincial. [laughs]

There were no dues. At least there didn't use to be. There was no publication, no proceedings. Three members had to nominate a person for membership, and then, as I said, only a few would be elected each year. So lots of people were nominated and not elected.

Hughes: The rule stipulating no publications was mainly to keep the meetings informal? Encourage reports on works in progress?

Reeves: The purpose of those meetings at that time was for people to lay out to the group something that they were doing, and then every effort was made to shoot it down, if there was any way to shoot that person down. I mean, those meetings were bloody and mean. You get the K. F. Meyers, the Albert Sabins, the Alex Langmuirs, and the Joe Smadels in a room and turn them loose, and it can get rough. Their joy in life was: if a person presents data and we can't shoot it down, it must be awfully damn good. The challenge was to break that person down and to show that the statistics were wrong, the epidemiology was wrong. They were a bunch of headhunters; they picked on each other, and they picked on any poor novice who showed up.

Hughes: That was a function of the personalities that happened to be involved?

Reeves: That's right. Nowadays their meetings are pretty calm, cool, and collected.

Hughes: Do they have a publication now?

Reeves: No. But for practical purposes they had the American Journal of Hygiene, which was under the epidemiology department of Johns Hopkins University as an outlet for publication. This is now the American Journal of Epidemiology. Many of the papers that were based on original research and findings presented at the American Epidemiology Society were published almost automatically in that journal. A publication didn't get into that journal that wasn't approved by the staff of the epidemiology department, because it was that department's journal. That's been changed some now.

To my amazement--and I don't remember the year; it was sometime in the late 1940s--I got a notice that I had been elected to membership in this society. Now, I can't help but believe that Drs. Hammon and Meyer had some influence. They must have nominated me, and I wasn't very old at that stage. It was pretty exciting to be nominated and elected. I gave a paper on our research at the first meeting after my election. All of the heat came down on me, and I just smiled my way through it.

Hughes: What kind of heat?

Reeves: Yelling at me, telling me I was wrong, didn't know what I was doing. I just yelled back at them and presented the data.

Hughes: Who yelled?

Reeves: Alex Langmuir, Albert Sabin, and others. Here was the latest new boy on the block, and I was the first entomologist--and as far as I know still the only one--whom they had elected. Everyone else in the organization was a physician. Some were microbiologists or biostatisticians, but for practical purposes it was a physician group. But by that time I had become inured. Physicians as such and big names didn't frighten me. [laughs] Well, I had to deal with Dr. Meyer and Bill Hammon on an everyday basis. I was giving papers at the APHA, tropical medicine meetings, and elsewhere. I was working with the Public Health Service and the army commission [the Virus Commission of the Armed Forces Epidemiological Board]. These same physicians weren't strangers to me. That probably also had something to do with my getting elected, but anyway it was very nice.



I stayed an active member and went to a meeting at least every three years, and frequently every year, until a few years ago, when I decided that this was ridiculous, because almost all the papers were now on chronic diseases, not on infectious diseases. They were as likely to be biostatistical as epidemiological papers. I decided I could become an emeritus member and forego the necessity of going to a meeting every three years, because there were other meetings I'd rather spend the money going to. Also, it would make room for more young people to become members. I was pleased to see several years ago that my oldest son was elected to membership. We may be the only father and son members.

Hughes: Do you think that AIDS and the problem with emerging viruses may change the emphasis away from chronic diseases?

Reeves: Not significantly. I think the big bucks are still going to be in the chronic diseases for a long time to come. In fact, some people think of AIDS as a chronic disease because of the long incubation and duration of the disease. Heart disease and cancer aren't going to go away, and these diseases are killing one hell of a lot more people than AIDS ever will, unless something really dramatic happens that I don't anticipate. There are a lot of chronic diseases, and they're going to continue to be the force that controls where research demands and money are going to be. When you look at the demographic projections of the population of the United States, it's going to be predominantly an old population, and we are going to have to find some way to resolve the health problems of those people or we're going to go bankrupt. You can't take care of these people with Social Security, Medicare, and such ways forever. You've got to decrease the health problems in that population some way. Either research will do it, or it's not going to happen and we're going to go broke.

This shift of mortality from infectious to chronic diseases has been happening for a long period of time in the U.S. The improvements in our environment and specific communicable disease control is why it's this way. Now, that doesn't mean that a new disease like AIDS or any other emerging infectious disease is not going to get a lot of attention, perhaps even more than it deserves. If it hits an age group or a social group that can make it a cause, it's going to have a big political and social impact very rapidly, and it's going to get attention. It's going to be politically necessary to give it attention. But the deaths from AIDS are not going to quickly change the age structure of the U.S. population. Now, I also don't happen to think I want to be here, or probably will be here, to find out whether I was right or wrong [laughs], but that's a separate problem.



### American Mosquito Control Association

Hughes: The American Mosquito Control Association.

Reeves: The American Mosquito Control Association (AMCA) is a national organization as contrasted with the California Mosquito and Vector Control Association (CMVCA). I have been quite active in both organizations, but I've never been an officer in either one. These organizations are largely made up of people who are practicing mosquito control; I mean, they're actually out there doing the mosquito control jobs. So I attend their meetings, present research findings to them, and at times am appointed to some of their committees.

I don't go to a lot of AMCA meetings, because I have limited money to go to national meetings and the American Society of Tropical Medicine has taken much of my time. About every five years the AMCA meets here in California with the CMVCA, so of course I then go to the meetings. And sometimes they will meet in Utah, Oregon, or some place nearby. I don't go to a lot of national meetings of other societies I belong to unless I've been invited to give a paper or make some special presentation. The mosquito control associations are extremely important to me because they represent the profession that carries into practice the findings of our research, whether it's what I'm doing on encephalitis, what somebody's doing with insecticides, or whatever the research might be.

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Hughes: Were you ever aware of tension between researchers, namely you, and the mosquito control people?

Reeves: Perhaps I misunderstood your question, but I really haven't perceived that type of feeling. I told you earlier how I was called on by the CMVCA to be their intermediary with the state health department. In fact, if there was such feeling, I doubt if I would have been given the Meritorious Service Award in 1981 by the CMVCA or the Medal of Honor for Distinguished Contributions to Mosquito Control and the Memorial Lectureship Award by the AMCA in 1982.

What you have to realize is that research in support of mosquito control is multidisciplinary. Entomologists can make important contributions to mosquito control, as can other sciences and professions. As examples, engineers are extremely important,

because they can solve control problems by modifying landscapes, such as salt marsh or whatever, and in this way build out mosquito breeding. Entomologists and toxicologists work together to improve toxicants for control, as do biologists, who are doing research on mosquito predators and biological insecticides to replace chemical toxicants. We epidemiologist-entomologists determine which mosquito species should be controlled because they carry encephalitis viruses.

Some mosquito abatement districts are doing a great deal of their own applied research. They should be, because it's difficult to get university people interested in some applied problems. Now, the districts may not call it research; they may call it vector management or something like that. A lot of the mosquito control people publish their research and observations in journals like the Journal of the American Mosquito Control Association, which used to be called Mosquito News. The AMCA decided that title was too informal. I'm sorry they changed the name, because I used to like to be introduced at the medical center [UC San Francisco] as a person who publishes a lot of his research in Mosquito News. The medical students would go into hysterics about that, thinking about publishing a paper for mosquitoes to read. It was and is a respectable journal; they have an editor, editorial board, and a panel of referees. But they changed the name to the Journal of the American Mosquito Control Association. That journal will contain applied science papers on new and novel approaches to mosquito control, but they also publish very detailed research papers.

I don't sense that there's a competition between researchers and mosquito control people. I think there is a feeling on the part of some mosquito control operators that the researchers aren't solving their problems fast enough and giving them new methods of approach or new materials to make their job easier for them. So what's new? If researchers answered every problem the minute it came to the forefront, pretty soon there wouldn't be any more need for research; everything would be answered. The problems faced by mosquito control aren't that simple and continue to pose serious challenges to the researcher.

I think the people in mosquito control recognize the importance of research and that researchers are trying to come up with new findings and new methodologies. At the same time, the mosquito control people tend to be critical of the people who want to study a mosquito just for the sake of studying mosquitoes, without any concern with control. They won't necessarily endorse the work of a person who uses the molecular level approach to mosquito problems unless they can see some immediate practical application. So I think the membership of CMVCA is sensitive



about where research money is being spent, especially if it is state money, and even try to influence the direction in which it is used. At the same time, I don't think there's any competition between researchers and control people, because they have quite different objectives.

Hughes: And, in general, two different educational backgrounds?

Reeves: Usually, yes, but not always.

Hughes: Where do most mosquito control people come from?

Reeves: They come from a wide variety of backgrounds. At this point in time, the specific training required to be manager of a mosquito control district is not dictated by a state law. A number of entomologists are managers of mosquito abatement districts, and some of these people have Ph.D.s in entomology. Other managers are engineers. There are managers who come out of the field of environmental health, whom we used to call sanitarians. They have bachelor's or master's degrees in environmental health.

Recently in California, when some managers retired the local board of trustees, who are appointed by local governments and run the organization, have felt that a long-term secretary or foreman was best qualified to be manager. These people knew what the district was about, knew the trustees, knew what the rules and regulations were, and the trustees felt they had a knowledge and experience sufficient to manage the field control crews. A number of the districts now are hiring women to be operators who are out spraying mosquitoes or doing the supportive entomological work. It used to be that MADs were mostly all-male organizations.

While it's perfectly possible to have a district that is well run by a person who has little scientific or technical educational background, I don't think that's usually true of the larger districts. Some of these organizations in California have budgets of \$3 million a year. That's a big business, and the manager is going to be managing a number of highly trained professional people in order to carry out the district's program. In addition, there are now laws that control the training that the technicians and operators have to have concerning insecticide formulations, how to use insecticides, mosquito biology, disease transmission, and so on. Mosquito control technicians must now pass state administered examinations to be certified. They have to take refresher courses each year to keep their training certification current. You don't want to have a manager whom you found on the street looking for a job.



In the final judgment, the board of trustees for each mosquito abatement district, who are citizens representing the people who live in the district, select the manager and formulate policy and budgets for their district. The board of trustees hire a person to run the district. Their judgment controls who is appointed and their tenure. This is generally true nationally as well as in California.

Hughes: Have you ever had problems having your work accepted by the mosquito control people?

Reeves: I don't know what you mean by "accepted." They'll accept our scientific facts. They may not like what we do or accept the recommendations that we make. They may not even think that we're doing research the way it should be done. At the same time, I think they understand that as a scientist, I have to make the decision as to what's going to be done and how it's going to be done. Some may disagree completely with what I choose to work on and think they know better than I do what I should be working on, primarily because they want research to be most helpful to them in their job. So the scientist is the one who's really on the spot if he wants their support. The manager of a district isn't on the spot, except he may not have the scientific information he'd like to have.

Hughes: A lot of your research has been very practical.

Reeves: Yes, that is true. The mosquito control people recognize that the university also has a role to carry out basic research. The basic research may be somewhat different than they'd like to have it be. However, the mosquito control districts have a voice in this. They're the ones who provide the funding. A district can provide funds to a researcher to do specific tasks. When the university goes to the state legislature and requests funds for mosquito research, the individual mosquito control agencies and the CMVCA are the lobby that can go to Sacramento to support the need for research by the university on mosquito-borne diseases and mosquito control.

Hughes: Which is more important, basic or applied research?

Reeves: I think they're both extremely important. When anyone says the molecular biology approach is the only approach to solve problems, then I think they're wrong. If I say that the field or biological investigative approach is the only way to solve these problems, then I am wrong. I think a dichotomy of basic versus applied research can be very harmful. If someone says that no research should be done on mosquitoes that is not directly applicable to their control and that basic biologic or taxonomic knowledge has

no use, I think they are wrong. Now, that's not a philosophy; it's just that both approaches can be very important.

Hughes: I'm sure there were junctures in your professional career where you could have chosen to go more towards basic science or more towards the applied.

Reeves: Every individual has to make those choices, and even more so today than they used to have to. Some scientists decide to go entirely into applied or basic; others succeed in maintaining a mix of the two.

### Physicians in Public Health

Hughes: How do people make such decisions?

Reeves: Well, I'll give you an example. World War II was over, I'd been teaching tropical medicine at the medical school, and I had my Ph.D. The dean of the medical school, Francis Smyth, came to me and said, "Bill, wouldn't you like to get an M.D. degree? With your background, what you've done researchwise, and the courses you've taken, you could complete all of the basic course requirements for a medical degree in two years." That was a very nice compliment. He said, "You should do that."

I thought about it very seriously, and I said, "No, I don't want to be a physician. You may want me to be a physician, maybe because you think I'd be a good physician, but I'm not interested in taking care of sick people. I want to do things in a different way, because I don't think that a doctor in an office or operating room is ever going to solve the health problems of a population. A doctor is seeing sick people, and a sick person frequently is a failure in my book. I want to be able to prevent people from getting sick. Then I will have done something that I wanted to do. I'm not against doctors taking care of sick people. When I'm sick, I want to see a doctor. I don't want to spend my life doing medicine, because I don't find it exciting, I don't find it challenging, I would find it repetitive, and I don't want to do the other things you'd want me to do to become a physician. I recognize the value of having that "union tag or card." I understand that I'll never be able to examine the person who has encephalitis and collect specimens from him, but that isn't what I want to spend my life doing. I find these other things exciting and interesting and challenging, and they raise many more questions in my mind." I've never regretted my decision.

Hughes: Why did he even suggest it?

Reeves: I think Dr. Hammon or Dr. Meyer probably suggested it to the dean. They thought, and I'm sure that in some ways they were right, that I would have an easier time in my career if I became a physician. Not that I'd necessarily ever use the knowledge from the medical training. Indeed, there are a lot of physicians showing up in public health who don't want to be practicing physicians.

Hughes: That's new, isn't it?

Reeves: Not really. We encountered that way back in the forties with the people we were retreading--the health officer physicians who did not have training in public health and who didn't want to be practicing physicians. They didn't want to be taking care of sick people; they wanted to prevent people from being sick.

Hughes: My impression is that in the early days, public health was largely an M.D. occupation. M.D.s, grant you, with a different orientation than the average M.D. But hasn't that changed?

Reeves: I don't think so. I think medical schools generally are still not getting people very excited about preventing diseases. I think they're much more concerned with training a surgeon, a G.P. [General Practitioner], a psychiatrist--almost any branch of medicine you want to mention. Actually, if you look at the organizations that have existed within medical schools, preventive medicine was an antipathy. Nobody wanted to "prevent medicine," so they changed the name to community health or something like that to make it more acceptable. There's certainly not a lot of pressure in the medical schools for students to go into preventive medicine or public health.

Hughes: I meant by my question to ask whether the composition of the field of public health changed so that nowadays the proportion of people with M.D. degrees is smaller.

Reeves: It's much smaller, and it goes even further than that. It's not even considered essential or important that a physician be the person in charge. If you want to extend it even further, look what's happened in hospitals. In the forties and earlier, the hospital administrator almost had to be a physician. Today, you go to most major hospitals, and the highest level in which you'll find a physician is the physician directly in charge of the medical aspects of the program, the doctor part of the program; but the person running the hospital is most likely not going to be a physician. The odds are against it. Hospital administrators now are being trained in business administration. We're lucky if they have had courses in public health, leave alone medicine.



When they have an outbreak of a nosocomial infection in the hospital that is knocking off people with staphylococci or some other infection, they recognize that they've got a public health problem. They ought to be controlling the disease. People should not be getting infected in the hospital. Now they are into a public health preventative problem within a medical facility.

Hughes: Do you think that's a bad trend?

Reeves: I'm not sure there is such a thing as good and bad trends.

Hughes: Can you give me a philosophy on career selection?

Reeves: I don't really think I have such a philosophy. As I've said many times, if I have a philosophy, it's related to the question my parents asked me originally, "How are you ever going to make a living being an entomologist? Who in the heck wants to hire somebody to be interested in bugs? Go into something else where you're sure of making a living." I made such a decision and persisted in going into entomology. However, I've told many young people, "Look, if you're not going to be happy doing what you're getting an education to do, go sell shoes, sell real estate. You can probably make more money than in a scientific career."

### The Job Market in Science

Hughes: Do you find that young people nowadays do want to enter entomology?

Reeves: Well, a lot of them may have wanted at some time to go into entomology; but if they do, they may not be able to find a job. If physicians can't make a living in private practice somebody's usually happy to have them run a clinic for them or be a staff member in a hospital.

The real problem with going into a biological area like entomology is that the job market may not be big enough to let you do exactly what you want to do. You'd better have a second field you are prepared in. People going into entomology may also take a master's degree in epidemiology or in a field related to environmental protection. They then have something to fall back on. The same is true for fields other than entomology. We recently have had a number of anthropologists getting a degree in epidemiology. They have a Ph.D. in anthropology, and they find out they can't get a job to do anthropology. At the same time, they realize that a cultural anthropologist may have a real

contribution to make in epidemiological studies, as do people from other social sciences. We get an increasing number of people with a Ph.D. or other doctoral degree who say, "I don't want to go to a state college and teach my subject out of a textbook." They find that if they study the sociological aspects of heart disease or cancer, there's a need for them as epidemiologists. We're giving these people the ability to apply their basic science training to epidemiology, as I did with medical entomology and parasitology. With such preparation, they can get into a broader field where there are more jobs.

### Entomologist or Epidemiologist?

Hughes: Dr. Reeves, do you look upon yourself as an entomologist, an epidemiologist, or something else?

Reeves: Yes. [laughter]

Hughes: Thank you very much. [laughter]

Reeves: It's an impossible question, because there's no profession that you can use as a category for a person who has become a jack-of-many-trades. So yes, I was trained in medical entomology and parasitology. I use that knowledge in teaching, and I use it as a tool in research. I make no pretense that I try to keep up in that field as far as all the literature is concerned. I have enough entomology to be able to do some effective work with insects in nature and to know when to bring somebody else in with additional tools. Bill Reisen is a current colleague who brings in additional approaches to our research.

When I went to school, there wasn't a course in virology on this campus, so how can I ever consider myself to be a virologist? I've never had any formal training in virology. Whatever I've picked up in the laboratory has been whatever was the tool of the moment. If I had to learn enough about it to use it in the laboratory, I did, but I may not understand the principles of virology. I don't make any pretense of being a virologist in the modern sense. Again, I know when to turn to a person in that field--namely, Jim Hardy.

I've already told you that I took a course in epidemiology early in my career, and I was teaching half the course at the same time I took it for credit. The course was on the epidemiology of infectious diseases; Dr. Hammon and I taught it. I had one course in biostatistics, which was really the only one offered in the

school at that time. So in some ways I may consider that I do not have sufficient formal training in epidemiology or in biostatistics to call myself an epidemiologist in today's world. A large part of my training was gained through practical experiences while working in the field and using what the tools of the moment were. I have now come to a point where I no longer can keep up with some of the methodologies, particularly in biostatistics, because I don't have the math background to do that. However, at the same time I believe that I am firmly based in the principles of epidemiology and can apply them to a variety of important problems.

So what I have wound up with is a lot of experiences from working with practical problems that required a basic knowledge in epidemiology, entomology, and virology, sufficient to function or to recognize the need to associate with someone who had the information I needed. Most of this was done by getting the team together that was necessary to do the job. If I did anything, it was to synthesize the knowledge and apply it.

I carry the title of professor of epidemiology, so that must mean that in some ways I'm an epidemiologist. I think I've made contributions in that field. Early on I made contributions enough for the American Society of Epidemiology to recognize the fact that I was an epidemiologist. It's a title that in the earlier years was sort of a self declaration: "I'm an epidemiologist." If people asked you, you said, "I'm an epidemiologist". Or if somebody else asked you, you might say, "Yes, I'm an entomologist; I study bugs." Or, "Yes, I'm a virologist, because I discovered some new viruses."

I was involved in the development of the curriculum in epidemiology at Berkeley from its first day. If you talk to Leonard Syme, he would say today that he still can't understand how and why this entomologist who was recruiting a staff in epidemiology ever brought him, a Ph.D. in sociology, into the program. He had never had a course in epidemiology, and I brought him here as a professor of epidemiology. I did it for a darned good reason, because a sociological approach to heart disease or other diseases was an absolutely essential area to be developed as part of our curriculum, and I thought he was the person to do it. I was right.

For a long time, in the other schools of public health, almost every person who came into epidemiology was a physician epidemiologist or a biostatistical epidemiologist. I felt we needed to broaden our base more than that to teach students what they needed. I may not have understood exactly what Syme was doing or how he would do it except by his serving as a model. He



hasn't tried to make himself an entomologist, and I'm not a sociologist, but we certainly share the common concern to learn why and how certain people in a population are selected to have a disease and to find a way in which we can prevent that disease. That continues to be our objective. So we needed a multidisciplinary faculty in epidemiology, as we did for our field research program. In our field program we have had entomologists, parasitologists, virologists, physicians, veterinarians, vertebrate zoologists, nurses, and biostatisticians.

So I guess if I had to have a name put on me--and it might not make some people too happy--I'd probably consider myself mostly an epidemiologist, because I consider it to be a sufficiently broad title to cover all my interests. At the same time, some of my entomological associates might want me to say that I should consider myself a medical entomologist.

Hughes: Because you're not a classical epidemiologist?

Reeves: I feel I am a classical epidemiologist and perhaps a classical medical entomologist. However, I'm not a specialist in many of the specialties within those fields.

Hughes: So whatever you choose, you're going to offend somebody.

Reeves: The last thing I want to do is to offend anybody by misrepresenting myself.

Hughes: I know, and I trust you don't.

Reeves: I don't want people to say I'm not being loyal to my field. I have an equal concern with the future of medical entomology, epidemiology, and virology. But maybe I'm not guilty of being the best representative for any one or all of those fields.

Hughes: Do you think it's as possible nowadays as it was when you began your career for a person to have a broad scientific outlook, to have the opportunity to work in a variety of fields?

Reeves: I think a person can make that opportunity if they're determined to do so. I think it's increasingly difficult for a person to avoid becoming more and more specialized, because you can't be a master of any of these fields unless you do specialize. People always seem to be looking for a person who is really the master of a single field. Epidemiological studies are becoming more and more biostatistical, and I fear that if you're not careful, they will eventually avoid the epidemiological, biological, and even the intuitive approaches to problems.

Hughes: Did it ever bother you that you didn't have formal training in some aspects of the problems you were working on?

Reeves: You can't help that, and it bothers you, but you also have to recognize that you can't spend your whole life just getting more training and more depth in all of the fields. As an example, a good high school student now enters college with a knowledge in many areas that weren't available when I was a graduate student. As a further example, when the new insecticides, DDT and the other organophosphorus compounds--malathion and parathion--came in, I was there and could have become a specialist in that area. However, I soon realized that people were expecting me to know all about DDT, the chemistry of this group of agents, all about how insects develop resistance through enzymes that will inactivate DDT, and the literature on the other chlorinated hydrocarbons. I soon realized I was going to spend the rest of my life reading like crazy, trying to keep abreast of this literature. I didn't have enough basic training in chemistry or genetics to understand it. I decided somebody else was going to be the expert on insecticides, not me. There are plenty of people who are experts on insecticides; I don't have to be. I know when I need them. If I'm in charge and I need that person, I'll get that person.

### Research Funding Problems

Hughes: And the granting organizations never protested?

Reeves: Yes, they protest. You may submit a research grant to study some new finding you have made, and they may say, "You shouldn't be getting interested in this, because you're not competent in this area." On the other hand, no one who is competent to study it has discovered it or seems to be interested in pursuing your finding. We have experienced this.

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Hughes: Can you give me an example?

Reeves: We discovered that mosquitoes can cure themselves of a virus infection, and we wanted to pursue that. Nobody else had discovered that they could cure themselves of infection, and we don't know the mechanism. It is an interesting problem to pursue.

Hughes: Why aren't you and your team competent to research that area?



Reeves: We were told it's because we're not that well trained in molecular biology and the approaches that are needed to solve the problem.

Hughes: Was it the molecular biology that they felt was missing?

Reeves: Yes. Now, there were other things. I think that in their opinion we asked for too much money, as the mosquito escaping virus infection was only one aspect of our proposal. Many people can't understand the amount of money it takes to support the field aspects of our work, the travel that's necessary to get out into the field and collect mosquitoes, or the staff that is required. You can't do our type of research for free. A strictly laboratory-oriented person doesn't understand that, but that's not something new.

To carry out a program of the magnitude and diversity that we were trying to do, we couldn't continue doing it unless we expanded our funding. We made an effort to continue to get enough money to do the variety of things we wanted to do on global warming, insects curing themselves of infections, what factors control vector competence, and so on. We tried to keep it as a package, because we thought there was an advantage to doing that. Our application didn't quite make the cutoff point for payment, so now we have to see whether we can rebuild the program by fragmenting it into smaller packages that individually cost less.

Hughes: Do you think it is a disadvantage nowadays to be asking for money for a field program?

Reeves: It definitely is a disadvantage unless it's a problem such as AIDS, and it can be a disadvantage there if development of a drug or vaccine becomes the emphasis in AIDS research.

Hughes: What is the thinking there?

Reeves: It's the magnitude of what you have to have, the diversity of people you have to have, and it's the relative importance in other people's minds of the diseases or problem you want to study. I can't say that encephalitis is a major disease problem that deserves headlines each year in California or in the United States. Many people don't accept the fact that research findings about encephalitis potentially have very broad applications to other major diseases that are mosquito borne. I think that historically many of the methodologies we've developed and our findings have had extremely wide application. People don't usually remember where the knowledge came from or that current research may have the same value.



Hughes: If one looks at the broader picture, your research is very relevant.

Reeves: Thank you. [laughs]

Hughes: Too bad I don't have any money to give you. [laughter]

Reeves: I say thank you, because I think so, too.

Hughes: Yes, but it's true.

Reeves: I wouldn't be doing the research if I didn't think so. You have to remember that I have been retired for five years, and I'm not supposed to be working. But I'm still finding that research is exciting and interesting. We have a current research group in which I have the highest respect for each individual. I'm trying to give them what support I can, but some days I'm not sure I shouldn't get out of the way completely. Maybe I'm even a handicap. I don't know. Perhaps people are not going to tell me to my face if I'm a handicap.

Hughes: I think you'd know.

Reeves: Well, you always wonder. But that's not a very good note with which to wind up this discussion. I've been very happy with what I've done, and I've gotten a lot of satisfaction out of it.

Hughes: Those aren't incidental qualities.

### Publication

Hughes: On an entirely different subject, namely publication, how did you determine the order of authorship? Specifically, whose name went first or who would be listed as an author?

Reeves: I realized the problem this could pose many years ago. I have taken a position, for better or worse, that a person who cannot stand up in front of an audience, present the paper, answer questions, and defend the paper should not be an author on a paper.

Now, a lot of people tell me that's unusually harsh and difficult. Still, I don't believe that people who are hired to do a particular job because they are good technicians should be authors because they did the job assigned to them. An author also should have made original contributions of methodology, ideas of

what ought to be done, and how they should be done. Finally, he or she should be able to present the data. If not, they don't deserve the recognition of being an author. A person who offers physical support, gives you a building to work in, or loans you a piece of equipment is really not relevant to the thinking aspect of the scientific contribution. I think that person's support should be acknowledged, but I don't think he usually deserves being an author on a paper.

Sometimes this has posed difficulties, because some people who are legitimate authors on the paper will say that the project couldn't have been done without the help of a certain person. I may say that the person was trained and hired to do that job but not as a research scientist. That person didn't contribute any original thoughts to the project or to interpretation of data. At the same time, I also have never adopted the position that because I got the money for the research it meant I automatically should be senior author or even an author on the resulting paper.

#### Choice of Senior Author

Hughes: How do you choose a senior author?

Reeves: The senior author, as far as I'm concerned, should be the person who contributed the major original idea or without whose participation the research couldn't have been done. They significantly implemented the study and did it on a professional basis. I mean, it was not just something that they were assigned to do. Now, this is the ideal situation.

I carry this philosophy one step further: I've never insisted that my name had to be on publications that came from a student thesis. I have only rarely had a student do a thesis that I was a major advisor on where it wasn't the student's idea to do that project and not my idea. Many students have worked around the fringe of the basic objectives of our research grants. They have picked research problems that we weren't necessarily going to do as the basic core of our research grant application. The student's project still can make very important contributions. The facilities for the research grant work are there to be used, and it is easy for the student to recognize the importance of their project and to do it.

Earlier I gave you the example of my student Dick Emmons, who is now the director of the state virus laboratory. He selected his thesis topic on Colorado tick fever and did an excellent job.

I'm acknowledged in his thesis for the support I gave as his advisor. That's fine. I'm not an author of any of the papers that resulted, and I didn't think I should be.

I also have had times that students have come to me of their own volition and said, "I want you to be an author on this paper with me." They wanted to do it because of thoughts or guidance I'd given them. Or perhaps I had actually worked with them on--you might say donated labor to--their project. Finally, they may have wanted my name to be associated with theirs in their biography.

Hughes: They were first author?

Reeves: Yes, always. No exception.

Hughes: Was that true when you were a young man starting off?

Reeves: No, it was not. That's why I've always done it. I never had a problem with it when I was a student, but I saw that other students did. What had a lot of effect on my adoption of this general policy was my experience when I worked with Drs. Hammon and Meyer. I never had any question that I got the full credit that I thought I deserved on every paper. The place that I wound up in authorship was right where it should have been. I never felt that I was cheated in any way, and if I had been, I would have objected loudly and strongly. In fact, I was allowed to base my entire Ph.D. thesis on the early field studies in the Yakima Valley, with full support from the entire research team.

Some people say, "If I'm in charge of a laboratory, I'm going to be an author on every paper that comes out of it." I don't agree with that view, and obviously everybody else doesn't agree with me. [laughter]

The reason I've insisted that students have so much input into the topic of their thesis and that they're not my slave labor is that I hope in their careers they're going to have the ability to develop ideas and methods of approach for their research. No better time to have the first opportunity to do so than in their thesis. However, as advisor I'll only let them screw up to a certain extent before I intervene, because if they continue they won't have a thesis. I think the most important part of their training is coming up with a problem and how to approach it, and having it be a topic important enough that it's the basis for a thesis. The doctoral theses develop new methodology and represent new scientific findings; they don't merely confirm something we already know.



### Pressures to Publish

Hughes: How much pressure did you feel to publish?

Reeves: I felt two pressures to publish. The first, as I've said, was that I did not believe in research being done and not having the results become available to other people. I think a scientist has a debt if he's going to do funded research and use facilities and time that are funded by tax money. The scientist also owes it to himself. I don't accept the explanation of failure to report research by, "Well, I just don't like to write papers," or "I did this for the sake of science." I don't accept those viewpoints. I feel that anybody who has the opportunity to involve himself in research owes it to the public to put the research results in the record. Then somebody else doesn't waste time doing the same thing in the future. I don't like reinventing wheels. That to me is a waste of the public's time, facilities, and money. I don't happen to think that society owes scientists a living just for the sake of science.

The second pressure was that if you don't publish, the source of funds is going to go away. The funding source will say, "Hey, your research grant application and progress reports said you would do this, and your reports say you did it, but you have not published it in a reputable journal; ergo, you're going to lose your sources of funding."

The third pressure was academic. It brings to mind the old saying, "Publish or perish," and that ain't no joke. In a university such as at Berkeley, if you do not publish, you might as well get the hell out of here or accept that you're going to be treated like an inert vegetable and not be promoted, even if tolerated. Nothing very good is going to happen in your life as far as your family or your own self is concerned. And you'd better be careful that your best research is published in refereed journals and is not buried in annual progress reports. Because if so, they're not going to count in your evaluation for merit increases or promotions.

At the same time, I could be perfectly willing to be in a university where I was publishing primarily for certain rewards and not just for tenure. I could be willing to give up tenure and put it on the line that I thought I was going to be productive enough in research to justify my continued presence and an increased salary. A lot of people in the university wouldn't be willing to do that. As a matter of fact, at one time, with tongue

in cheek, I even recommended to an Academic Senate committee that we completely reverse the process so that assistant professors could have tenure for life if they were willing to live on that level of salary and not have research facilities or graduate students. They would just teach and do committee work. They would have tenure, but if they wanted to go to the higher salary levels and have equipment, supplies, space, graduate students, and so on, then they could give up tenure and say, "I'll put my name on the line. I do want to do research for promotion, and I'm confident I can do it. If I don't, then I won't be here anymore."

Hughes: How did the Academic Senate committee respond?

Reeves: The people on the committee who wouldn't have put their names on the line thought it was a horrible idea, and other people on the committee who enjoyed research and a sense of competition thought it was a great idea. In fact, they recognized that they could have more space, facilities, and other benefits besides higher salaries. Needless to say, my suggestion was tabled. Actually, the committee was abolished.

Another importance of publication is the satisfaction that you get yourself. At the same time, I don't agree with people who publish just to see how many publications they can have their name on. I also don't believe in the business of duplicate publications on the same data, which a lot of people make a living out of, or try to.

Hughes: Don't the granting organizations see through that?

Reeves: Usually. It depends on who's doing the reviews.

### Personal Background

Hughes: Dr. Karl Johnson described you as "one of the few true citizen scientists I know."<sup>1</sup> Would you like to comment?

Reeves: I don't know what he meant. [laughter] I thought most scientists in the United States were citizens of the United States except for a few foreigners.

Hughes: I think he means in the sense of contributing to society at large.

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<sup>1</sup>Karl M. Johnson. Professor William C. Reeves: scholar, teacher, and friend. American Journal of Tropical Medicine and Hygiene 1987; 37:3S-7S.

Reeves: It's very nice of Karl to say that, and I remember when he said it. I think this represented in part Karl's joy of playing with words, and it was a nice way to give an accolade, which I appreciated. However, I don't know exactly what Karl means sometimes when he uses that sort of terminology, so I really can't enlarge upon his statement. Some days I think Karl feels I'm sort of a Woody Allen, the character in the TV program "Cheers."

Len Syme calls me Woody sometimes because of my vocabulary, my approach to some problems, and my naivete about some things. But still, it's an act that seems to work. [laughter] I think some of my colleagues are somewhat intrigued that my background is somewhat incompatible with my position. You know, I came from a farm background, from a relatively poor family. I didn't come from a cultured background, and I still don't consider myself to be particularly cultured in the sense of the arts or even some of the sciences; but still, I found a place where I can make a contribution, and I'm very happy to do that. At times I may get to a sort of earthy ground level.

Hughes: I think your success attests in a very favorable way to American society--the fact that a person with ability regardless of background can get to the top.

Reeves: It would have been much more difficult in Britain, France, Germany, or even Australia. I've never been in a situation where my background was raised as a critical question, and yet I believe there are educational institutions where this could have become a real problem.

Hughes: Institutions in this country?

Reeves: Yes. I think it's obvious what I'm talking about. Some university communities still are very conscious of family backgrounds and social status. People must not only be competent in their field, but they also must have a family heritage for social purposes.

Wil Downs is a person I've had close association with who was very much in the same boat as I. He did not come from a particularly cultured or wealthy background, but he certainly was able to achieve everything that a man could want in his career. That's one of the things that he and I shared and talked about in addition to our mutual interests in science and fishing.



The Role of Serendipity

Hughes: Has serendipity played any role in your career?

Reeves: I think it does in anybody's career, because on occasion you suddenly find yourself in a place where everything makes sense and is important, but you didn't plan it that way--if that's what you mean by serendipity. I frequently found myself in such positions, where I was given an opportunity to investigate an epidemic and found I had an opportunity to observe a unique set of circumstances. You didn't plan it; you're sitting there, and the event is occurring. Isn't that serendipity? You recognize that you have a unique opportunity because something unusual is happening, and you can take advantage of it to get new knowledge and an understanding of why it happened. That to me is serendipity, because you didn't plan it.

Hughes: But you had the sense to take advantage of the circumstances.

Reeves: The sense to recognize the opportunity to observe a natural experiment that's going on underneath your nose. I'll give a very elementary example.

The whole west side of Kern County was a desert, and suddenly the government decides to put a big irrigation system into this desert and make it farming land. It was nothing but sagebrush and any other sort of bush you wanted, with rattlesnakes, jack rabbits, and kangaroo rats in abundance. There were hardly any people and very few birds there, and yet we knew the area was going to become rich agricultural land producing big income for individuals just because they were putting water in there. They were going to take all that sagebrush out; they were going to completely destroy the rodent habitat, and they were going to make it primarily an avian and human habitat. And they were going to take it from being a tick and flea habitat and make it a mosquito habitat. So we were able to watch that area over a five- to ten-year period and to observe what changes took place with reference to viruses and how fast they occurred. We didn't plan the development for that purpose; they just handed it to us.

As a separate example, the fact that I was in Berkeley when I originally got into this field almost comes under serendipity. In 1941, when Dr. Hammon was looking for somebody who had some interest in mosquitoes and viruses to join a team, he didn't even know me and didn't have any idea whether I'd be able to do the job or not, and I didn't either. I didn't even know what the problems were going to be. I think that comes under the category of

happening to be in the right place at the right time and recognizing that you can do something. Is that serendipity?

Hughes: I think so.

Reeves: It seems to me that a very large part of anything I've done has not been because I was so darned smart that I could manipulate the situation so that it was going to happen. It was keeping my eyes open and recognizing what the chances were.

Hughes: But with a large dose of native wit and considerable education.

Reeves: You've got to keep a sense of humor about it [laughter] if that's what you mean by "wit." Otherwise, you're doomed to failure. When things don't turn out the way you want, don't say, "Well, gee, tough luck." That's what Karl Johnson frequently has made reference to about me. If things didn't come out the way I'd anticipated they would, I didn't just say, "I give up." I tried to find an explanation for it. My agility sometimes astounded everybody. [laughs] But I say that in a very friendly fashion, as you realize. Sometimes I don't understand how some people do what they do or get some things done. I watch them with interest to see if I can learn from them.

### Hobbies

Hughes: Do you care to say something about your hobbies?

Reeves: I don't know whether they're hobbies, avocations, or what they are, but I do have interests. We talked earlier about my interest in hunting and fishing. A lot of people object to what I enjoy doing, because they don't believe in killing anything. I don't believe in killing things for the sake of killing them, but I do get a great deal of satisfaction and pleasure in observing what's going on and trying to interpret the situation so that I'm successful. But I also am not a "game hog;" I don't kill most of the fish that I catch. If I don't have some real use for them, I put them back. I'm never really happy in an urban or an indoor environment. I'm not happy unless I'm out of doors doing something. So you might say being out of doors is a hobby with me and that I'm happy whether I'm doing fieldwork or sports. I guess that's why I look on my field research projects as fun. It is really a hobby. I continue to do it after retirement, so now I know it must be a hobby.

My interest in orchids is one that sort of generated itself. My eyes were popped out of my head the first time I got into the tropics and could see orchids blooming in trees and realized that they didn't depend upon being soaked with water twenty-four hours a day, and that if you did, it killed them instead of making them happy. Then, to my surprise, I found that some of my closest associates in arbovirology, Wil Downs, Tommy Aitken, and Pedro Galindo, shared my interest in orchids.

I thought it would be fun to raise them, but I didn't want to get involved in their genetics, I didn't want to get into pollination, I didn't want to get into creating new species or hybrids. I just liked to have them around. I have now three greenhouses where I can look at them and repot them. When I'm in the greenhouse, I am in a miniature "out of doors." I also have done an awful lot of brick and cement work around my home, building walls and things. When I'm doing that sort of thing, I don't have to be thinking about anything. I'm busy with my hands and getting some exercise.

I don't do my deepest scientific thinking while I'm out there fishing or doing another hobby. I'm just enjoying myself, and those are the sorts of hobbies I like. I don't have a hobby of collecting stamps or baseball cards. I don't even have any serious hobbies that are related in any way to science except to consider field research a hobby. I'm not a person interested in arts and theater; I can enjoy them, but I'm not particularly concerned about them. So I guess if I'm going to be serious about a hobby, it has to be something that deals with the outdoors. I would love to play a lot more golf than I do, just because I would find it challenging and interesting. But with the schedules that I've had in my life and the prohibitive cost of playing golf in the Bay Area, I just play golf when on vacation.

### Generating Scientific Ideas

Hughes: When do you do your deepest thinking?

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Reeves: I would guess that I probably do more thinking when I'm in bed and can't sleep, or I'll wake up with an idea. You can think through things much more rapidly and without interruption in that circumstance. I also find myself thinking about problems on what I call incidental time. I might be sitting here writing a paper or preparing a lecture, and suddenly I get a research idea



unrelated to what I'm doing and had better make a note of it so I don't forget it--that sort of a thing.

Hughes: So it's an unconscious sort of thing?

Reeves: No, it's not unconscious. I wake up with this on my mind for some reason, and then I do a lot of rapid thinking. Now, a lot of times I'll do something like that and have what seems to be a great idea, and if I don't make a note of it, I may find I can't remember what it was the next morning. It's sort of like a dream. You've had a dream, and you know you've had a dream, but you can't remember what it was.

Hughes: In the morning do you find that they actually are good ideas?

Reeves: Sometimes they're good, and sometimes they're horrible. [laughter] Sometimes I drive people crazy; the first thing in the morning, I'm phoning them, saying, "I woke up last night and realized that we ought to be thinking about this or that."

I also think a lot of ideas come when you're communicating with other people who have common interests with you. Bruce Eldridge and I were just on this two-day trip to Oregon studying snow mosquitoes, and we pretty well ran through the whole gamut of possible research topics that we might want to do in our future research. Some of the ideas one of us would shoot down, and they were discarded. Others we agreed would be worth developing further and then stored them away for future consideration. We covered a lot of miles with nothing to interfere with our discussion. At other times we might cover many miles in silence and no communication until we got to the field area.

You never know where the ideas come from. I wasn't concerned at all about global warming. I sort of looked at the stuff about it that was in the newspapers, television, and magazines, but I wasn't interested until 1987, when an EPA group invited me and Marilyn Milby to attend a conference in Washington, D.C. First they told us the projections for global warming. Then they said, "What's global warming going to do to the diseases you're working with?" I said, "I don't have any idea." They said, "Well, we can't accept that. You must have some idea." So with the parameters they had given us, I began to throw out a few ideas, and the next thing I knew, we had a project on this problem. But you know, I was stimulated by their original question. To come back to your earlier question: was this serendipity?

At the same time, I've also spent a lot of time farming out ideas. I go off to be a consultant with some outfit and see an opportunity to give them an idea that I had stored away for a

while and never got around to doing anything about. I feed it to them, and it gets done. I like to do that in such a way that they think it's their idea. I don't expect to be on their publication or to participate in the project. If it gets to be the other guy's idea, it's more effective, because they're more enthusiastic about it. [laughter] Nobody likes somebody coming along and saying, "Now, you do this and you do that."

### Contributions

Hughes: What do you consider your most important contribution?

Reeves: [laughing] Sitting here, as you and I are, I could say it was enduring over forty hours of taping an oral history.

Seriously, I don't think you have asked me a fair question, because I'm not sure there is any single most important contribution that I've made. I have the feeling I've made some contributions, and I guess if I have to state one it would be this: I was given the opportunity in the early 1940s to study the epidemiology of western equine and St. Louis encephalitis. I had no idea this was going to be a lifelong focus of interest. The encephalitis epidemics that were going on in the thirties and early forties were very interesting. We didn't know where they had come from, what caused them, or how they were transmitted. I was able to get in on the ground floor to answer these questions, and then for fifty years I was able to follow it through by constantly probing into research areas that originally you never conceived would be a problem. The subject is so big that sometimes it has been a very ephemeral problem. But I was given an opportunity to pursue that problem for a fifty-year period. The reason I call this my most important contribution is because of the satisfaction it has given me.

If there is to be a most important contribution, it has to be that broad. I mean, there's no single thing that I would designate. And then, at the end of my career, to know I had been privileged to work with colleagues that I respect. In 1990 we published a summary of a very large part of that research in what I hope will be considered to be a definitive publication.<sup>1</sup> I

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<sup>1</sup>William C. Reeves in collaboration with S. Monica Asman, James L. Hardy, Marilyn M. Milby, and William K. Reisen. 1990. Epidemiology and Control of Mosquito-borne Arboviruses in California, 1943-1987. Calif. Mosq. Vector Control Assoc., Inc., Sacramento, Calif. pp. 1-508.

can't think of any individual contributions I have made that would have more significance than that overall experience. I've had a lot of hypotheses and ideas on how to pursue them, and some panned out and some didn't. Methodologies were developed that have had very broad acceptance and use by my peers. Ground rules were set down about what was necessary to prove that a vector or vertebrate host was important in disease maintenance.

There are an awful lot of these research problems that I would approach differently if I were doing them over again. If I had a choice, I'd still like to do the same sorts of things again. I just feel fortunate that I was able to do it. Now, that isn't a very good or specific answer to your question. If I have one regret concerning the research, it is that I do not believe we have resolved a final answer to the question of how western equine and St. Louis encephalitis viruses survive through the winter. We have spent a great effort but only partially answered that question. It is left for a future generation.

Now, I'm sure if you asked twenty individuals, if they cared at all, what they consider to be my most important contribution, you'd get twenty different answers. That came out when a group of close friends and peers gathered to honor me on my retirement in April 1987.<sup>1</sup> Each of the nine speakers focused on a different topic because it was an area of most interest to them, and I had contributed knowledge in that area.

Hughes: If we've done our work, we've covered those in some way.

Reeves: If you can accept that broad a response, I prefer not to narrow things down more than that.

Hughes: I can accept it.

Reeves: I assumed you mean scientific contributions, because I also put considerable importance on having been allowed to dedicate my life to developing an educational program here in the university in public health and epidemiology that I think has been successful and has influenced a lot of people's careers. But I find teaching a frightening experience.

Hughes: Why is that?

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<sup>1</sup>1987 Symposium. The Epidemiology of Mosquito-borne Virus Encephalitides in the United States, 1943-1947. Amer. J. Trop. Med. Hyg. 37: 1S-100S.



Reeves: It just makes me nervous. My adrenalin goes up for each lecture, and it's such a challenge.

Hughes: But you're so good at it.

Reeves: I never have felt that way. I always feel I've done an insufficient job and haven't done the preparation I'd like to do. One of the difficulties of a university career and one of the advantages is that you're expected to do at least three full-time jobs. I think there are a lot of people who find that extremely difficult, and I never found it easy. To briefly expand on this: at Berkeley you're expected to do an outstanding job of research, an outstanding job of teaching, and an outstanding job at community service. To really do an outstanding job in any one of those areas is more than a full-time job. Yet you're expected to do all three and to do them well. That's a constant challenge, and I've always felt that I didn't do as good a job as I could have done on any one. But maybe the system is right and I'm wrong; I don't know.

### Regrets

Hughes: Any regrets?

Reeves: No, any regrets that I have deal with the current situation: the university's trouble with budget because of the financial state of California with its budget. Things are happening to the Berkeley campus that I don't like at all. They're having to retract on programs at a time when I don't think that's the direction they should be going. I feel about that like I did when I was dean. The times dictated the fact. I couldn't do anything to construct new academic programs when I was dean; it was hold the fort, and that's where we are now. They are holding the fort.

We're not in a time of expanding science, and there isn't a lot of public appreciation for science. Too many diversions are occurring that preclude society from focusing on this particular problem or that particular problem. There is inadequate funding available to the university in the foreseeable future for adequate development of new ideas, new fields, or for that matter to continue some established programs. I think it's a very difficult time, and I don't know what's going to happen to the academic and science programs on this campus. I'm happy that I came into arbovirus research at a point in time when we didn't know where the diseases came from, we didn't know how they were transmitted, and we didn't know how to control them. Through our research we

obtained enough information to answer those questions, and now we're coming around to the point where we're looking forward to the next epidemic, when these infections will be looked at by some as an emerging disease, because they haven't posed a major health problem in their memory.

An emerging disease may be one we know awfully well and know it is going to emerge because society by law or by some other means says you can't control it because other ecological concerns are more important. The primary concern of a major segment of our society is returning environments to their original state or not wanting any so-called unnatural substances in the environment. It's not very satisfying to think you've gone in a great big circle, from knowing nothing about a disease or how to control it to where you feel you damn near know almost everything. Except you know you really never know everything. And then what's going to happen? Are you going to go through a whole new epidemic cycle? It could happen. If you paralyze our society, you know an epidemic is going to happen, but you cannot utilize your knowledge to prevent it. I regret that situation exists.

Hughes: Do you think we're in danger of that?

Reeves: Sure I do. I wouldn't still come to work every day if I didn't feel that way. I think anybody who doesn't think it can happen is foolhardy, because it's happened in many other eras and ages historically. Historically, diseases have knocked off many pretty advanced societies. The Roman Empire collapsed in large part because of malaria and other societies because of plague. To think that we're through with epidemic diseases of an infectious nature that are vector borne is just foolhardy. To say that we won't know anything about the diseases that recur or emerge from cycles maintained in nature I think is equally irresponsible.

I'm not losing a lot of sleep about it. I'm more likely to wake up with some other idea in my mind. I still call Bill Reisen in Bakersfield, Jim Hardy in Berkeley, or Bruce Eldridge in Davis and say, "You've got to do this or that." [laughter] I have to be very careful that I'm not guilty of running around telling people, "You've got to do this" and "You've got to do that," because I don't have any power over them any more. [laughs] I never really felt that I had power over them anyway. It still has to be we who will be doing it.

No, I have no regrets, except to realize I have spent forty-some hours taping an oral history and now realize I still have to read all this damned thing once it's transcribed. [laughter] I am sure it will be like a lot of my publications, Sally. I go back to read some of my publications and say, "Did I write that?"

Or, "I didn't say that, did I?" It isn't that I necessarily think it's wrong; it's like it isn't mine. It's somebody else's. I can read it in a completely disconnected fashion, and I'm sure I'm going to feel pretty much that way about this project.

Sally Smith Hughes, I must tell you it was my privilege to be interviewed by you. You have an excellent set of ears, much patience, and an unusual ability to lead one down memory lanes. You are a "pro." Even when the tape machine failed or the transcriber couldn't understand me, it has been a great experience. I have no regrets that we undertook the task.

Hughes: I'm going to say thank you, Dr. Reeves. I feel equally privileged. [laughter]

Transcriber: Elizabeth Kim

Final Typists: Elizabeth Kim and Judy Smith



## TAPE GUIDE--William C. Reeves

## Interview 1: November 14, 1990

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## FAMILY TREES FOR WILLIAM C. REEVES &amp; MARY JANE MOULTON

William Reeves  
early 1800s  
New Jersey

Wm. Claude Reeves + Virginia Muchmore  
1835-1890                      ????-1907  
New Jersey                      Ohio  
(crossed Panama &      (teacher of vocal  
became very ill      and instrumental  
with measles; had      music)  
2 brothers)

Lewis Albert Brant + Ollie Pryor  
Ohio                      Ohio  
(school teacher;  
believed 3rd grade  
was enough for  
girls, including  
his own daughter)

Wm. Claude Reeves, Jr.  
1882-1967  
Ohio  
(only child)

+

Alice Bessie Harriet Brant  
1890-1974  
Tennessee  
(one brother)

(moved to California)

William Carlisle Reeves  
1916-  
Riverside, CA  
(only child)

William Carlisle Reeves  
+ -- 3 sons -- Robert Flay Reeves  
Terrence Moulton Reeves

Mary Jane Moulton  
1917-  
Riverside, CA  
(one brother)

Flay Edwin Moulton  
1893-1975  
Missouri  
(one sister;  
moved to California)

+

Esther Johnson  
1895-1979  
Minnesota  
(7 sisters, 1 brother;  
moved to California)

Steven Hilton + Margaret Matthew  
Moulton                      1870-1931  
1870-1925                      Missouri  
Missouri

(moved to Minnesota)  
Charles Augustus + Caren Ilaug  
Johnson/Rocstad\*                      1870-1932  
1868-1923                      Norway  
Norway

\* Name was Rocstad but he worked for a family named Johnson and took their name, as was customary where he lived; had several older brothers.





## BIOGRAPHICAL SKETCH

**NAME:** William Carlisle Reeves

**BORN:** December 2, 1916 - Riverside, California

**EDUCATION:** Riverside Polytechnical High School, 1934  
Riverside Junior College, A.A., 1936  
University of California, Berkeley, B.S. (Entomology) 1938  
University of California, Berkeley, Ph.D. (Medical  
Entomology and Parasitology) 1943  
University of California, Berkeley, M.P.H. (Epidemiology)  
1949

**EMPLOYMENT:** Agent, USDA, Forest Insect Investigations - Summers 1939, 1940  
Teaching Assistant Medical Entomology and Parasitology, U.C.  
Berkeley, 1939-41  
Entomologist, Research Assistant and Research Associate, U.C. San  
Francisco, Hooper Foundation, 1941-49  
Lecturer in Tropical Medicine, U.C. San Francisco, 1944-46  
Lecturer, School of Public Health, U.C. Berkeley, 1946-49  
Associate Professor Epidemiology, School of Public Health, U.C.  
Berkeley, 1949-56  
Professor Epidemiology, School of Public Health, U.C. Berkeley,  
1956-87  
Dean, School of Public Health, U.C. Berkeley, 1967-71  
Head, Program in Epidemiology, Department of Biomedical and  
Environmental Health Sciences, School of Public Health, U.C.  
Berkeley, 1975-85  
Retirement to Professor of Epidemiology Emeritus, U.C. Berkeley,  
July 1, 1987

**PRESENT AND PAST SERVICE TO FEDERAL GOVERNMENTAL AGENCIES:**

Member, Viral Commission, Armed Forces Epidemiological Board  
Consultant in Medical Entomology to Surgeon General, U.S. Army  
Consultant, Ecological Investigations Program, Center for Disease  
Control, USPHS  
Consultant to Director, National Communicable Disease Center, USPHS  
Member, Viral and Rickettsial Disease Study Section, USPHS  
Member, Epidemiology and Biometry Research Training Program  
Advisory Committee, USPHS  
Chairman, Arbovirus Research Reagent Committee, NIAID, USPHS  
Member, Advisory Committee to Surgeon General, USPHS on Public  
Health Service Foreign Quarantine Activities  
Member, Ad hoc Advisory Committee on the Implementation of a  
Professional Career Development Program in Global Community  
Health, Surgeon General, USPHS  
Member, Planning Committee on the Role of U.S. Biomedical  
Institutions in International Health Programs, Fogarty  
International Health Center, USPHS  
Member, National Advisory Allergy and Infectious Disease Council  
NIH

Chairman, Ad hoc Study Group on Viral and Rickettsial Diseases,  
U.S. Army Medical Research and Development Advisory Panel  
Chairman, Ad hoc Study Group on Medical Entomology, U.S. Army  
Medical Research and Development Advisory Panel  
Member, U.S. Army Medical Research and Development Advisory  
Committee  
Member, Committee on Microbial Threats to Health, Institute of  
Medicine, National Academy of Science

SERVICE TO INTERNATIONAL AGENCIES:

Consultant, Pan American Health Organization  
Member, WHO Expert Panel on Virus Diseases  
Member, Advisory Scientific Board of Gorgas Memorial Institute of  
Tropical Medicine and Preventive Medicine  
Director Emeritus, Gorgas Memorial Institute

SERVICE TO CALIFORNIA STATE GOVERNMENT:

Chairman, Vector Control Advisory Committee, California State  
Department of Health  
Consultant in Epidemiology, California State Department of Health

MEMBERSHIP AND OFFICES IN PROFESSIONAL ASSOCIATIONS AND SOCIETIES:

American Society Tropical Medicine & Hygiene, Past Council Member -  
President and Editorial Board  
American Committee, Arthropod-borne Viruses - Past President and Ad hoc  
Treasurer  
International Northwest Conference Diseases of Nature Communicable  
to Man - Past President  
Alumni Association, School of Public Health, U.C. Berkeley - Past  
President  
American Public Health Association; Past Council Member and Past  
Secretary, Epidemiology Section  
American Association Advancement of Science - Fellow  
American Entomological Society  
American Epidemiology Society  
American Association for Epidemiological Research  
American Mosquito Control Association  
Society for Vector Ecology  
Wildlife Disease Association  
U.S.-Mexico Border Public Health Association  
Western Branch-American Public Health Association  
Editorial Board, Journal of Medical Entomology  
Sigma Xi  
Delta Omega  
Alpha Zeta  
Phi Sigma

AREAS OF SCIENTIFIC CONCERN:

Study and elucidation of the biological factors that control the



spread of viral diseases by arthropod vectors, with emphasis on those infections that are spread by mosquito vectors that pose public health and veterinary health problems.

Teaching concentrated in the area of infectious disease epidemiology.

#### HONORS AND AWARDS:

Richard Moreland Taylor Award for Achievement in Arbovirology, 1973- American Committee on Arbovirology of the American Society of Tropical medicine and Hygiene  
Distinguished Teaching Award 1980-81, University of California, Berkeley  
Delta Omega Society, National Merit Award for Outstanding Achievement in Public Health, October 1980  
Honorary Fellow, Queensland Institute of Medical Research, February 1981  
California Mosquito and Vector Control Association Meritorious Service Award, 1981  
American Mosquito Control Association Medal of Honor for Distinguished Contributions to Mosquito Control, 1982  
Memorial Lectureship Award, American Mosquito Control Association, 1982  
John Snow Award, American Public Health Association, Epidemiology Section, 1982  
Thomas Francis, Jr. Memorial Lecture, University of Michigan, 1983  
The Berkeley Citation for Distinguished Achievement and for Notable Service to the University of California, 1987  
Medal for Distinguished Civilian Service to the United States Army, 1987  
Symposium held in honor of William C. Reeves, PhD: "The Epidemiology of Mosquito-borne Vial Encephalitides in the United States, 1943-1986". University of California, Berkeley, April 11, 1987  
California State Senate Resolution No. 127 of Commendation, 1987  
Establishment in 1987 of the The William C. Reeves Young Investigator Award of the California Mosquito and Vector Control Association (an annual selection)  
Distinguished Service Award, The Society for Vector Ecologists, 1987  
Outstanding Achievement in Medical Entomology Given in Honor of Harry Hoogstraal - American Committee on Medical Entomology of the American Society of Tropical Medicine and Hygiene, 1987  
The Walter Reed Medal-Awarded by the American Society of Tropical Medicine and Hygiene in Recognition of Meritorious Achievement in Tropical Medicine, 1987  
Gold Headed Cane Award by the American Veterinary Epidemiology Society; American Veterinary Medical Association, 1988  
Alumnus of Year, School of Public Health, University of California, Berkeley 1991  
Selected for development of a "Personal Oral History," Bancroft Library, University of California, Berkeley 1990





## APPENDIX C

## Complete List of Publications

WILLIAM C. REEVES, PhD

NO.	AUTHOR	TITLE	PUBLISHED
a	Reeves	Newer developments in knowledge of insect hosts and vectors of western equine and St. Louis encephalitis.	Proc & Papers 12th Ann Conf Calif Mosq Control Assoc <u>12</u> :23-37, 1941.
b	Reeves	The arthropod-borne virus encephalitides of the Pacific area.	Proc & Papers 13th Ann Conf Calif Mosq Control Assoc <u>13</u> :8-11, 1944.
c	Reeves	Mosquito control and encephalitis in California.	Proc & Papers 18th Ann Mtg Florida Anti-Mosquito Assoc, 1947, pp 59-65.
1	Reeves	The mosquito genus <u>Mansonia</u> Blanchard in California	Pan-Pacific Entomol XVII (1):28 (Jan) 1941.
2	Reeves	The genus <u>Orthophodomyia</u> Theobald in California.	Pan-Pacific Entomol XVII (2):69-72 (Apr) 1941.
3	Hammon, Reeves, Brookman, Izumi, Gjullin	Isolation of the viruses of western equine and St. Louis encephalitis from <u>Culex tarsalis</u> mosquitoes.	Science <u>94</u> :328-330, 1941
4	Reeves, Hammon, Izumi	Experimental transmission of St. Louis encephalitis virus by <u>Culex pipiens</u> Linnaeus.	Proc Soc Exp Biol Med <u>50</u> :125-128, 1942.
5	Hammon, Reeves	<u>Culex tarsalis</u> Coquillett a proven vector of St. Louis encephalitis.	Proc Soc Exp Biol Med <u>51</u> :142-143, 1942.
6	Hammon, Reeves, Brookman, Izumi	Mosquitoes and encephalitis in the Yakima Valley, Washington: I. Arthropods tested and recovery of western equine and St Louis viruses from <u>Culex tarsalis</u> Coq.	J Infect Dis <u>70</u> :263-266, 1942.
7	Hammon, Reeves, Izumi	Mosquitoes and encephalitis in the Yakima Valley, Washington: II. Methods for collecting arthropods and for isolating western equine and St. Louis viruses.	J Infect Dis <u>70</u> :267-272 1942.

NO.	AUTHOR	TITLE	PUBLISHED
8	Bang, Reeves	Mosquitoes and encephalitis in the Yakima Valley, Washington: III. Feeding habits of <u>Culex tarsalis</u> Coq., a mosquito host of the viruses of western equine and St. Louis encephalitis.	J Infect Dis <u>70</u> :273-274, 1942.
9	Reeves, Hammon	Mosquitoes and encephalitis in the Yakima Valley, Washington: IV. A trap for collecting live mosquitoes.	J Infect Dis <u>70</u> :275-277, 1942.
10	Hammon, Reeves Brookman, Gjullin	Mosquitoes and encephalitis in the Yakima Valley, Washington: V. Summary of case against <u>Culex tarsalis</u> Coq. as a vector of the St. Louis and western equine viruses.	J Infect Dis <u>70</u> :278-283, 1942.
11	Hammon, Reeves Gray	Mosquito vectors and inapparent animal reservoirs of St. Louis and western equine encephalitis viruses.	AJPH <u>33</u> :201-207, 1943
12	Hammon, Reeves	Laboratory transmission of St. Louis encephalitis virus by three genera of mosquitoes.	J Exp Med <u>78</u> :241-253, 1943
13	Hammon, Reeves	Laboratory transmission of western equine encephalomyelitis virus by mosquitoes of the genera <u>Culex</u> and <u>Culiseta</u> .	J Exp Med <u>78</u> :425-434, 1943.
14	Reeves, Hammon	Feeding habits of the proven and possible mosquito vectors of western equine and St. Louis encephalitis in the Yakima Valley, Washington.	Am J Trop Med <u>24</u> :131-134, 1944.
15	Reeves	<u>Culex tarsalis</u> and other mosquitoes as vectors of the virus encephalitides of western North America.	Summary of the dissertation submitted in partial satisfaction of the requirements for the degree of Doctor of Philosophy, U. of Calif, Oct 1943
16	Hammon, Reeves Irons	Survey of the arthropod-borne virus encephalitides in Texas with particular reference to the lower Rio Grande Valley in 1942.	Texas Rep on Biol & Med <u>2</u> :366-375, 1944.



NO.	AUTHOR	TITLE	PUBLISHED
17	Reeves	Preliminary studies on the feeding habits of Pacific Coast anophelines.	J Nat Malaria Soc <u>III</u> (4): 261-266, 1944.
18	Hammon, Reeves	Epizootology of western equine type encephalomyelitis: Eastern Nebraska field survey of 1943 with isolation of the virus from mosquitoes.	Am J Vet Res <u>VI</u> (20):145-148, 1945.
19	Hammon, Reeves	Recent advances in the epidemiology of the arthropod-borne virus encephalitides.	AJPH <u>35</u> :994-1004, 1945
20	Hammon, Reeves Benner, Brookman	Human encephalitis in the Yakima Valley, Washington, 1942: With 49 virus isolations (western equine and St. Louis types) from mosquitoes.	JAMA <u>128</u> :1133-1139, 1945.
21	Hammon, Reeves	Certain bacteriostatic agents added to sera used in diagnostic tests for neurotropic virus infections.	Proc Soc Exp Biol Med <u>60</u> :84-88, 1945.
22	Hammon, Reeves, Galindo	Epidemiologic studies of encephalitis in the San Joaquin Valley of California, 1943, with the isolation of viruses from mosquitoes.	Am J Hyg <u>42</u> :299-306, 1945.
23	Hammon, Reeves, Burroughs	Japanese B encephalitis virus in the blood of experimentally inoculated chickens.	Proc Soc Exp Biol Med <u>61</u> :304-308, 1946.
24	Reeves	Observations on the natural history of western equine encephalomyelitis.	Proc 49th Ann Mtg US Livestock Sanitary Assoc Dec 1945, pp 150-158.
25	Reeves	Suggestions regarding <u>Culex tarsalis</u> control in encephalitis areas.	Proc & Papers 14th Ann Conf Calif Mosq Control Assoc <u>148</u> :82-83, 1946.
26	Hammon, Reeves, Izumi	St. Louis encephalitis virus in the blood of experimentally inoculated fowls and mammals.	J Exp Med <u>83</u> :163-173, 1946.
27	Hammon, Reeves	Western equine encephalomyelitis virus in the blood of experimentally inoculated chickens.	J Exp Med <u>83</u> :163-173, 1946.

NO.	AUTHOR	TITLE	PUBLISHED
27a	Reeves, Hammon	Laboratory transmission of Japanese B encephalitis virus by seven species (three genera) of North American mosquitoes.	J Exp Med <u>83</u> :185-194, 1946.
28	Hammon, Reeves, Cunha, Espana, Sather	Isolation from wild bird mites ( <u>Liponyssus sylviarum</u> ) of a virus or mixture of viruses from which St. Louis and western equine encephalitis viruses have been obtained.	Science <u>107</u> :92-93, 1948.
29	Reeves, Hammon, Furman, McClure Brookman	Recovery of western equine encephalomyelitis virus from wild bird mites ( <u>Liponyssus sylviarum</u> ) in Kern County, California.	Science <u>105</u> :411-412, 1947.
30	Reeves, Mack, Hammon	Epidemiological studies on western equine encephalomyelitis and St. Louis encephalitis in Oklahoma, 1944.	J Infect Dis <u>81</u> :191-196, 1947.
31	Hammon, Mack, Reeves	The significance of protection tests with the serum of man and other animals against the Lansing strain of poliomyelitis virus.	J Immunol <u>5</u> :285-299, 1947.
32	Rosen, Reeves, Aarons	<u>Aedes aegypti</u> on Wake Island.	Proc Hawaiian Entomol Soc XIII(2):255-256, 1948.
33	Reeves, Brookman, Hammon	Studies on the flight range of certain <u>Culex</u> mosquitoes, using a fluorescent-dye marker, with notes on <u>Culiseta</u> and <u>Anopheles</u> .	Mosq News <u>8</u> :61-69, 1948.
34	Reeves	A final summary of flight range studies on <u>Culex tarsalis</u> and notes on wild bird malaria in Kern County.	Proc & Papers 16th Ann Conf Calif Mosq Control Assoc <u>161</u> :58-59, 1948.
35	Tigertt, Hammon, et al.	Japanese B encephalitis: A complete review of experience on Okinawa, 1945-1949.	Am J Trop Med <u>30</u> :689-722, 1950.
36	Reeves	The present status of knowledge of mosquito ecology in California.	Proc & Papers 18th Ann Conf Calif Mosq Control <u>181</u> :59-61, 1950.
37	Reeves	Yakima, Washington, controls mosquitoes and flies at no cost -- why can't we?	Proc & Papers 18th Ann Conf Calif Mosq Control Assoc <u>182</u> :13-15, 1950.

NO.	AUTHOR	TITLE	PUBLISHED
38	Hammon, Reeves	Interepidemic studies on arthropod-borne virus encephalitides and poliomyelitis in Kern County, California, and the Yakima Valley, Washington, 1944.	Am J Hyg <u>46</u> :326-355, 1947.
39	Reeves, Washburn Hammon	Western equine encephalitis control studies in Kern County, California, 1945: I. The effectiveness of residual DDT deposits on adult <u>Culex</u> mosquito populations.	Am J Hyg <u>47</u> :82-92, 1948.
40	Hammon, Reeves, et al.	Western equine encephalitis control studies in Kern County, California, 1945: II. An evaluation of the effectiveness of certain types of mosquito control including residual DDT on virus infection rates in <u>Culex</u> mosquitoes and in chickens.	Am J Hyg <u>47</u> :93-102, 1948.
41	Brookman, Reeves	A new name for a California mosquito (Diptera: Culicidae).	Pan-Pacific Entomol XXVI (4):159-160, 1950.
42	Hammon, Reeves, Sather	Japanese B encephalitis virus in the blood of experimentally inoculated birds. Epidemiological implications.	Am J Hyg <u>53</u> :249-261, (May) 1951.
43	Reeves	The encephalitis problem in the United States.	AJPH <u>41</u> :678-686, 1951.
44	Reeves	Field studies on carbon dioxide as a possible host simulant to mosquitoes.	Proc Soc Exp Biol Med <u>77</u> :64-66, 1951.
45	Reeves, Rudnick	A survey of the mosquitoes of Guam in two periods in 1948 and 1949 and its epidemiological implications.	Am J Trop Med <u>31</u> :633-658, 1951.
46	Hammon, Reeves, Sather	Western equine and St. Louis encephalitis viruses in the blood of experimentally infected wild birds and epidemiological implications of findings.	J Immunol <u>67</u> :357-367, 1951.



NO.	AUTHOR	TITLE	PUBLISHED
47	Reeves, Hammon, Lazarus, Brookman, McClure, Doetschman	The changing picture of encephalitis in the Yakima Valley, Washington.	J Infect Dis <u>90</u> :291-301, 1952.
48	Hammon, Reeves	California encephalitis virus, a newly described agent. Evidence of natural infection in man and other animals.	Calif Med <u>77</u> :303-309, 1952.
49	Hammon, Reeves, Sather	California encephalitis virus, A newly described agent. II. Isolations and attempts to identify and characterize the agent.	J Immunol <u>69</u> (5):493-510, 1952.
50	Reeves, Hammon	California encephalitis virus a newly described agent. III. Mosquito infection and transmission.	J Immunol <u>69</u> :511-514, 1952.
51	Bellamy, Reeves	A portable mosquito bait-trap.	Mosq News <u>12</u> :256-258, 1952.
51a	Reeves	Report on the current status of research and knowledge on the arthropod-borne viral encephalitides in the United States, 1952.	Not published. Duplicated by CDC, USPHS, Feb 1953.
52	Reeves	Quantitative field studies on a carbon dioxide chemotropism of mosquitoes.	Am J Trop Med Hyg <u>2</u> :325-331, 1953.
53	Simons, Stevens, Reeves	Some epidemiological observations on tularemia in California, 1927-1951.	Am J Trop Med Hyg <u>2</u> :482-494, 1953.
54	Scrivani, Reeves, Brookman	Duration of activity of western equine encephalitis neutralizing antibodies in <u>Aedes nigromaculis</u> and <u>Culex tarsalis</u> .	Am J Trop Med Hyg <u>2</u> :457-463, 1953.
55	Brookman, Reeves	New records of mosquitoes from Lower California, Mexico, with notes and descriptions.	Ann Entomol Soc Am <u>46</u> :236, 1953.
56	Reeves	Possible recent introductions of mosquito vectors of human disease in the Central Pacific.	Proc 7th Pac Sci Congr VII:371-373, 1953.
57	Reeves	The knowns and the unknowns in the natural history of encephalitis.	Proc & Papers 21st Ann Conf Calif Mosq Control Assoc <u>21</u> :53-55, 1953.

NO.	AUTHOR	TITLE	PUBLISHED
58	Reeves, French, Marks, Kent	Murray Valley encephalitis: A survey of suspected mosquito vectors.	Am J Trop Med Hyg 3:147-159, 1954
59	Herman, Reeves, McClure, French, Hammon	Studies on avian malaria in vectors and hosts of encephalitis in Kern County, California: I. Infections in avian hosts.	Am J Trop Med Hyg 3:676-695, 1954.
60	Reeves, Herold, Rosen, Brookman, Hammon	Studies on avian malaria in vectors and hosts of encephalitis in Kern County, California: II. Infections in mosquito vectors.	Am J Trop Med Hyg 3:696-703, 1954.
61	Rosen, Reeves	Studies on avian malaria in vectors and hosts of encephalitis in Kern County, California: III. The comparative vector ability of some of the local <u>Culicine</u> mosquitoes.	Am J Trop Med Hyg 3:704-708, 1954.
62	Reeves, Sturgeon, French, Brookman	Transovarian transmission of neutralizing substances to western equine and St. Louis encephalitis viruses by avian hosts.	J Infect Dis 95:168-178, 1954.
63	French, Reeves	A group of viruses isolated from naturally infected mosquitoes collected in the Murray Valley area of Victoria and New South Wales.	J Hyg 52:551-562, 1954.
64	Reeves, Hammon, Doetschman, McClure, Sather	Studies on mites as vectors of western equine and St. Louis encephalitis viruses in California.	Am J Trop Med Hyg 4:90-105, 1955.
65	Reeves	Epidemiological aspects of encephalitis under field conditions.	Proc & papers 23rd Ann Conf Calif Mosq Control Assoc, 1955, pp 48-50.
66	Nani, Hollis, Reeves	The action of hyaluronidase enzyme on St. Louis and western equine encephalitis viruses in the chick embryo.	J Infect Dis 97:219-226, 1955.
67	Hammon, Sather, Lennette, Reeves	Serological response to Japanese B encephalitis vaccine of children and horses immune to St. Louis virus.	Soc Exp Biol Med 91:517-521, 1956.

NO.	AUTHOR	TITLE	PUBLISHED
67a	Reeves	BOOK REVIEW: Buxton, PA. The Natural History of Tsetse Flies, An Account of the Biology of the Genus <i>Glossina</i> (Diptera). London, HK Lewis & Co., Ltd, 1955.	Am J Trop Med Hyg <u>5</u> :343, 1956.
67b	Reeves	BOOK REVIEW: Carpenter SJ and LaCasse WJ. Mosquitoes of North America (North of Mexico). Berkeley, UC Press, 1955.	Am J Trop Med Hyg <u>5</u> :344, 1956.
68	Dow, Reeves, Bellamy	Field tests of avian host preference of <u>Culex tarsalis</u> Coq.	Am J Trop Med Hyg <u>6</u> :294-303, 1957.
69	Furman, Reeves	Toxic bite of a spider, <u>Cheiracanthium inclusum</u> Hentz.	Calif Med <u>87</u> :114, 1957.
70	Reeves	Arthropods as vectors and reservoirs of animal pathogenic viruses.	In: Hallauer C & Meyer KF (eds), Handbuch der virus-forschung, v 4 (Supplementary v 3). Vienna, Springer-Verlag, 1957, pp. 177-202.
71	Reeves, Bellamy, Scrivani	Relationships of mosquito vectors to winter survival of encephalitis viruses: I. Under natural conditions.	Am J Hyg <u>67</u> :78-89, 1958.
72	Bellamy, Reeves, Scrivani	Relationships of mosquito vectors to winter survival of encephalitis viruses: II. Under experimental conditions.	Am J Hyg <u>67</u> :90-100, 1958.
73	Reeves, Hutson, Bellamy, Scrivani	Chronic latent infections of birds with western equine encephalomyelitis virus.	Proc Soc Exp Biol Med <u>97</u> : 733-736, 1958.
74	Reeves (Moderator)	PANEL SESSION: Where are we going in mosquito control?; Chemical, physical, and biological control - what and how.	Proc & Papers 26th Ann Conf Calif Mosq Control Assoc, 1958, pp 40-45.
75	Hayes, Bellamy, Reeves, Willis	Comparison of four sampling methods for measurement of <u>Culex tarsalis</u> adult populations.	Mosq News <u>18</u> :218-277, 1958.



NO.	AUTHOR	TITLE	PUBLISHED
75a	Casals, Reeves	Arthropod-borne animal viruses.	In Viral and Rickettsial Infections of Man. 3rd Ed. Rivers TM & Horsfall FL Jr (eds), Philadelphia JB Lippincott Co, 1959, pp 269-283.
76	Reeves	The next step in encephalitis research in Kern County.	Proc & Papers 27th Ann Conf, Calif Mosq Control Assoc 27:97-99, 1959.
77	Reeves, Renteln	Determination of mosquito attack rates by interview of Kern County, California residents.	Mosq News 19:274-279, 1959.
77a	Reeves	Mosquito vector distribution and movements.	WHO/Arth. Virus Dis./33 29 August 1960.
77b	Reeves	Vaccine Development.	WHO/Arth. Virus Dis./35 2 Sept. 1960.
78	Reeves	Overwintering of arthropod-borne viruses.	In Prog Med Virol, v 3, New York, Karger & Basel, 1961, pp59-78.
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223	Reeves (republication of #52)	"Classic paper" Quantitative field studies on a carbon dioxide chemotropism of mosquitoes.	J Am Mosq Cont Assoc. 6: 208-712, 1991.
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228	Eldridge, Lanzaro, Campbell, Reeves, Hardy	Occurrence and evolutionary significance of a California like encephalitis virus in <u>Aedes squamiger</u> Diptera, Culicidae.	Am J Med Ent 28:645-651, 1991.
229	Emmons, Dondero, Milby, Hui, Murray, Theller, Ennik, Wilson, Pitstick, Hardy, Presser, Reeves, Barrett, Ascher	Surveillance for anthropod-borne viral activity and disease in California during 1989.	Proc & Papers 58th Ann. Conf. CMVCA. 58:1-11, 1990
230	Eldridge, Campbell, Hardy, Reeves, Lanzaro, Presser	Recent studies of California and Bunyamwera serogroup viruses in California.	Proc & Papers 58th Ann. Conf. CMVCA. 58:17-19, 1990

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234	Riesen, Meyer, Milby, Presser, Emmons, Hardy, Reeves	Ecological observations on the 1989 outbreak of St. Louis encephalitis in the southern San Joaquin Valley of California	J Med Entomol <u>29</u> :472-482, 1992
235	Campbell, Reeves, Hardy, Eldridge	Seroepidemiology of California and Bunyamwera serogroup infections in humans in California	Am J Epid <u>136</u> :308-319, 1992
236	Kramer, Reeves, Hardy, Presser, Eldridge,	Vector competence of California mosquitoes for California encephalitis and California encephalitis-like viruses	Am J Trop Med Hyg <u>47</u> :562-579, 1992





**SCHOOL OF PUBLIC HEALTH  
University of California, Berkeley**

**CHRONOLOGY OF SELECTED EVENTS**

- 1911-1944: Public health is curriculum within Department of Hygiene, University of California, Berkeley. Robert Legge and John Nivison Force are long-time chairmen.
- 1930s: Dr. Karl Meyer heads joint efforts of University of California at San Francisco, University of California at Berkeley, and Stanford to address California's need for school of public health.
- 1938: Meyer creates graduate curriculum in public health for retraining public health officers and sanitarians, sponsored by University of California and Stanford, supported by federal funds. Closed in 1939 when funds withdrawn.
- 1942: Northern California Public Health Association appoints committee on establishing a school of public health, chaired by Dr. William Shepard.
- 1943: Presentation to state legislature (Arnstein, Higby, Shepard) and passage of legislation, signed by Governor Earl Warren, authorizing a school of public health.
- December Board of Regents approves establishment of U.C. School of Public Health at Berkeley.
- 1944: Board of Regents funds School housed in Life Sciences Building. Department of Hygiene merges with School.
- 1944-1946: Walter Brown, Dean.
- 1945: Special course in sanitation for Hospital Corps, U.S. Navy.
- 1946: University of California at Los Angeles Department of Public Health founded, accredited by American Public Health Association. Undergraduate curriculum used as model for other schools of public health. Focus on development of graduate programs. Students mainly from the field of practice.
- William McD. Hammon, Dean.
- 1946-1951: Edward S. Rogers, Dean.

1947: Granting of first doctor of public health degree.

1948: Expansion to T-4 building--administration and graduate programs.

1949: Institute on Evaluation of Public Health Practices held for staffs of health departments in the western region of U.S.

Sanitary Engineering Research Laboratory (SERL) created by act of legislation.

Reaccreditation for master of public health and doctor of public health degrees by American Public Health Association.

1951: Loyalty Oath controversy--a few public health faculty resign.

1951-1967: Charles E. Smith, Dean.

1954: Coordinating Committee for School of Public Health and California State Health Department on cooperative activities and participation.

1955: Dedication of Earl Warren Hall, Cancer Genetic Laboratory, and library.

Window Rock Navajo Project--training of community health workers.

1956: Training Program in Chronic Disease.

Sanitary Engineering Research Laboratory becomes organized research unit.

1957: Naval Biosciences Laboratory, commissioned by Secretary of the Navy in 1950, becomes organized research unit within School of Public Health.

1958: Federal Aid to Schools of Public Health, specialty training grants and traineeships in addition to state stipends which were part of 1935 Social Security Act.

1959: Partnership with western branch of American Public Health Association for continuing education for health professionals and technical personnel.

Participation in planning health services and sanitation needs for the Olympic Winter Games in Squaw Valley, Placer County.

Public Health Training for Medical Students--three months training and placement with California Department of Public Health.

1960: First Ph.D. granted.

Statewide administration revised to establish separate school of public health at University of California at Los Angeles. University of California at Berkeley School of Public Health maintains association with University of California at San Francisco.

1961: Epidemiology Program accredited for preventive medicine residency for M.D.s. University of California at Berkeley first school of public health to receive such accreditation.

1962: Dr. William Stiles becomes director of studies for the Peace Corps Program at Berkeley.

Undergraduate public health education moved to state universities.

1963: First students enrolled in combined two-year dietetic internship and Public Health Nutrition Program leading to RD, MPH.

1967-1971: William C. Reeves, Dean.

1969: Legislative Bond Issue for Health Sciences Facilities Construction.

Unrest on campus caused by war in Vietnam leads to student involvement in university committees, curriculum revision, and increased focus on urban problems.

1970: Innovative program on minority recruitment and concern with minority problems.

Neighborhood Health Centers Seminars begun.

1971: Budget retrenchment.

Reorganization of academic programs--executive committee and five divisions.

1971-1982: Warren Winkelstein, Dean.

1973: Reevaluation of curriculum and self-evaluation by programs.



1974: Reaccreditation by American Public Health Association.

Reorganization of academic programs into two departments:  
 Department A--Social and Administrative Health Sciences  
 Department B--Biomedical and Environmental Health Sciences.

1976: Inauguration of Extended Degree Program.

Joint degrees established for MD-MPH and MS-MPH (Nutrition).

1980: SERL renamed Sanitary Engineering and Environmental Health Laboratory (SEEHL)

1982: Health and Medical Sciences Program administratively related to School of Public Health.

Community Oriented Primary Care Project begins.

First career fair--joint activity of School of Public Health, alumni, and community.

1982-1991: Joyce C. Lashof, Dean.

1983: Reaccreditation by American Public Health Association.

MPA-MBA Program established.

1983: Tenderloin Seniors Organization Project begins.

1984: Initiation of University of California at Berkeley Wellness Letter.

1987: Closure of Naval Biosciences Laboratory.

1991- : Patricia Buffler, Dean.

1992: SEEHL renamed Environmental Engineering and Health Science Laboratory.

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### "Centennial History Project, 1954-1960,"

Includes interviews with George P. Adams, Anson Stiles Blake, Walter C. Blasdale, Joel H. Hildebrand, Samuel J. Holmes, Alfred L. Kroeber, Ivan M. Linforth, George D. Louderback, Agnes Fay Morgan, and William Popper. [Bancroft Library use only]

### "Thomas D. Church, Landscape Architect," two volumes, 1978.

Volume I: Includes interviews with Theodore Bernardi, Lucy Butler, June Meehan Campbell, Louis De Monte, Walter Doty, Donn Emmons, Floyd Gerow, Harriet Henderson, Joseph Howland, Ruth Jaffe, Burton Litton, Germano Milano, Miriam Pierce, George Rockrise, Robert Royston, Geraldine Knight Scott, Roger Sturtevant, Francis Violich, and Harold Watkin.

Volume II: Includes interviews with Maggie Baylis, Elizabeth Roberts Church, Robert Glasner, Grace Hall, Lawrence Halprin, Proctor Mellquist, Everitt Miller, Harry Sanders, Lou Schenone, Jack Stafford, Goodwin Steinberg, and Jack Wagstaff.

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Postgraduate Research Histologist, the Cardiovascular Research Institute, University of California, San Francisco, 1966-1969; medical historian conducting the NEH-supported History of Medical Physics Project for the History of Science and Technology Program, The Bancroft Library, 1978-1980.

Presently Assistant Research Historian in the Department of History of Health Sciences, University of California, San Francisco, and an interviewer on medical and scientific topics for the Regional Oral History Office. The author of The Virus: A History of the Concept, she is currently interviewing in the fields of health maintenance organizations, virology, public health, ophthalmology, and molecular biology/biotechnology.



















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